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How should enquiry be conducted? This basic question is faced at some time or other by every scientist—be he natural or social. Here is a formulation of principles of enquiry common to scientists—notions which initiate and guide the course of a line of research—and a grand strategy of enquiry is proposed. It is suggested that this theoretical framework can be used to investigate why scientists have these patterns of enquiry.

WHAT DO SCIENTISTS DO?

by Joseph J. Schwab

The University of Chicago

"Man is by nature metaphysical and proud."
CLAUDE BERNARD

AIMS AND PROBLEMS

CERTAIN debates among scientists perennially reflower in their literature and their symposia and perennially fail of resolution. These debates concern the way in which enquiry should be conducted. Patterns of actual enquiries exhibit the same variety and persistence. Different scientists do different things. A mode of enquiry discredited by one scientist, dismissed at one time, discarded in one science, reappears and is fruitful in other hands, at other times, or in other sciences.

Three further points suffice to bound our problem. In the course of their professional lives, a few scientists change their habit of enquiry or the position they defend but many do not so change. Second, the practices and positions encountered (and the arguments in their defense) are remarkably constant from science to science and from epoch to epoch. Third, the order in which different modes of enquiry appear is *not* constant across different fields, different subject matters, different problems. What in one science is a starting point, in another is a middle or a climax.

To take account of these matters, we make four suppositions: (1) the alternative patterns of enquiry accessible to the man of science are few in number and capable of formulation; (2) a relatively stable personal preference contributes mightily to the view of a scientist about what ought to constitute his science; (3) these personal preferences represent a configuration of personality types among scientists which is relatively stable over time and sciences; (4) this personal factor and ephemerals of circumstance rather than inexorables of logic or of history often determine what is better or best for a given science or scientist at a given time.

What follows is the fruit of an effort to realize the first of these suppositions. It is also a framework through which to investigate the others. It consists of a scheme of the decision points in the process of enquiry and of the choices available at each such point.

The decision points and choices summarize what I have seen in some 4,000 scientific papers written by European and American scientists over the past five centuries in the biological sciences, the physical sciences and three "behavioral" sciences. Most of the papers are from the last two centuries, from the biological sciences (including clinical medicine and psychiatry) and psychology.

THE DECISION POINTS

The six decision points, briefly sketched below, obviously do not include all the points which exist in and around enquiry. I have not considered decisions involved in larger operations for which enquiry is only a ploy; e.g., careers toward administrative and political functions; careers of popular authority and entertainment; the keeping of academic posts and the advancement of

salary. I have also omitted such obvious and preliminary choices as that of science as a career and of a subject field in science.

Of the six decision points, three occur seriatim in the course of enquiry. Two others are labor-divisional choices in which some one phase of enquiry is chosen for repetition, eschewing the remainder of the process. The sixth represents an *O* choice which will replace our initial pretense that all scientific enquiries originate in efforts to make a contribution to knowledge of a subject-matter.

The brief descriptions here are intended to serve only as a preliminary guide. Their full sense will be expanded later.

Point 1. Invention or selection of a form of principle for enquiry. Five forms of principle are available, three with subdivisions.

Point 2. Judgment of proposed principles relative to enquiry *in medias res*. Four criteria are used. None is wholly ignored, as a rule. Choice consists of the relative weighting given each of the criteria.

Point 3. Selection among alternative sets of specific terms for detailed enquiries. Two criteria are used. Unlike the criteria of Point 2, these two criteria are often weighted in ratios approaching 1:0.

Point 4. Selection of "stable" or of "fluid" enquiry; that is, the pursuit of conclusions with terms hypostasized or pursuit of conclusions as tests of the terms of the enquiry.

Point 5. Selection of one phase of enquiry for repeated endeavor. Although enquiry can be divided into at least five tasks capable of being executed independently of executing others, only three are commonly discriminated for choice by the working scientist.

Point 0. Displacement of "knowledge of the subject-matter" by other desiderata determining researches pursued. Two alternatives are conspicuous.

THE FORMS OF PRINCIPLE FOR ENQUIRY (DECISION POINT 1)

We shall use "principle" to stand for the ideas which initiate and guide any planned activity. "Principle of enquiry" thus stands for the notions which initiate and guide the course of a line of research. The biologist who wonders "which of the virus strains causes distemper?" uses *cause* as one part of his principle and notions about the taxonomy of microorganisms, the relation of invaders to the body's economy, and of disease "entities" as other parts.

A principle of enquiry in this sense may arise from a doctrine consciously known and espoused by the scientist or it may be simply his habit, his unexamined way of recognizing his subject matter and his problems. Whatever its status in this respect, it remains, for our purposes, a principle of enquiry.

We are similarly indifferent to the original reference among philosophical commonplaces of a principle of enquiry. It may originally specify the nature of things, of method, of "mind" or knowledge without affecting its status for us as a principle of enquiry.

A group of notions achieve the status of principle of enquiry when they succeed in *bounding* and *analyzing* a subject matter so as to make it fit for enquiry. Bounding is exemplified when, say, a primitive psychology conceives "learning" as the retention of nonsense materials under conditions which minimize or exclude motivational factors and interpersonal relations. It is seen when economists discriminate a "free market"; when factors of shape and medium are excluded from kinematics; when the distinction of chemical and physical change restricts the subject-matter of chemical enquiry. It is seen again when a primitive physiology conceives its "organism" as that which (a) stops at the skin, (b) during maturity, (c) under "normal" conditions.

The analytic function of principles consists in identifying the meaning-units or meaning-elements which are to be treated in enquiry into the subject-matter. Thus a primitive physiology treats its organism as constituted of organs and their functions. The principles of another physiology identify homeostases and their supporting mechanisms as the elements of research. A third (within a different bounding) conceives the organism as a chemical-energetic system indefinitely interpenetrating an environment.

More disparate forms of principle are seen in the Freudian personality with its psychic organs and functions, by contrast to the Sullivanian in which "I" am the intersect of my friends and enemies and they, in turn, are the intersects of me and others.

The first specific service of principles to

the course of enquiry is to provide it with terms in which to couch its problems. Thus, for our primitive physiology, the problem of enquiry is of the form, "What is the function of Organ *X*?" or, "What organ performs function *Y*?".

By way of the terms of the problem, the principle then determines what shall constitute the data required by the enquiry and outlines the procedure which will elicit the required data. Thus our physiologist must know what alterations in the behavior of the organism ensue upon suppression of a part and he is instructed to remove an organ and contrast the deprived animal with a normal one.

Finally, the principles of an enquiry restrict the form which knowledge of the subject will take by indicating how the data are to be interpreted. Thus, an organ-function physiology leads to a catalog of assertions of the form, "The function of *X* is *Y*."

Of principles in this sense, we find five kinds: reductive, rational, holistic, anti-principled and primitive. Reductive, holistic and anti-principled principles are each represented by subspecies. (The historian of philosophy will find none of these unfamiliar. Plato, Aristotle, Augustine, Plotinus, Comte, Mill, Mach, Whitehead, *et al.*, appear to have influenced a certain number of scientists—or to have read a certain amount of modern scientific literature.)

Reductive principles

Reductive principles are the grey flannels of enquiry; serviceable and wearable almost anywhere by anybody. They rest on the notion that things are as they are because of what they are made of. A subject of enquiry is treated, not as a thing which is, but as a something constituted. The scientific account is sought in the constituents.

Atomic reduction. If one insists on recognizing *orders* of phenomena, the stage is set for atomic reductive principles. The world is seen as literally *compounded*, in the style of a nest of Chinese boxes. Thus physical particles constitute the organizations called chemical. Chemicals constitute each physiological. Society is the structure of physiologicals.

Comte, of course, is the familiar spokesman for such a view. It is in deference to him that I have made the hierarchy read as it does. We would currently need to state it thus: physicals, chemicals, biologicals, psychologicals, socials.

Says Comte (1943), ". . . to study social phenomena one must start from a thorough knowledge of the laws pertaining to the individual." This states the paradigm which atomic reductions copy or outdo. Atomic reduction would embark upon an enquiry by more-or-less abandoning the subject of interest in favor of a study of the behavior and properties of its immediate constituents. Thus the behavior of human groups is treated as a function of their physiology; physiological knowledge is sought in terms of chemistry; and chemical phenomena are explained in terms of the masses, charges, positions and motions of physical units. Pavlov's *physiological* reflexes as the determining constituents of behavior and the multi-enzyme system as the determining constituent of the organism are modern cases in point. In general, constituents are treated as determinative of the constituted and never the reverse.

Two varieties of this doctrine can be distinguished, though they do not deserve the privilege of a nomenclature. On the one hand, there is the variety which departs from Comte's paradigm. In this belligerently held version, the subject of interest is abandoned as the subject-of-enquiry. It is enough to know the constituents in their virgin state—not as constituents but as solitary items. From their properties seen in that condition, says this extreme view of reduction, we can infer, and predict, the organizations into which they might enter and the character that would result.

In the more temperate version, the subject of interest is not abandoned. It is treated as serving two necessary functions. Through it, one finds the properties and behaviors which are to be accounted for by a study of constituents. Second, it is only by way of behaviors exhibited in the con-

stituted order of phenomena that one can detect some of the most important properties of the constituent elements, since some of these properties will be potencies exhibited only in unions and combinations. Thus, it is in a study of the properties of chemical compounds that the chemist locates many of the problems he proposes to solve by study of their elements; and it is in the processes of combination and separation that important properties (e.g., valence) of the elements are seen.

Modern reductions depart from the Comteian pattern in still another way. The reduction may leap several steps at once down the Comteian hierarchy to rest on some constitutive factor held to be determinative of the behavior of all or several "orders" of phenomena. (And the determining constituents may be immaterial.) We have, for example:

> "The problem of the physical sciences is thus finally determined; to reduce the natural phenomena to unalterable, attractive and repulsive forces whose intensity depends upon the distance.
>
> ". . . Its task will be complete when the reduction of phenomena to simple forces is complete, and when at the same time, it can be proved that the given reduction is the only possible one which the phenomena admit. Then this reduction would have been proved to be the necessary conceptual form of the theory of nature; so there would then be justified as well the ascription to it of the objective truth." (von Helmholtz, 1847, p. 1)

In these several versions of reduction, the constant mark consists in the fact that causal efficacy is wholly located in the chosen constitutive elements. The constant mark of atomic reduction consists in the fact that the efficacious elements are treated as of a different order from the constituted subject.

An atomic reductive bias in the scientist (especially its belligerent version) is often used to relegate ultimate responsibility for the soundness of knowledge to the scientist of the lower order of phenomena while the investigator of the higher order maintains an amateur status which yet permits him to flaunt before his non-reductive colleagues a prestigeous knowledge they do not possess. A great deal of this was visible in physiology before biochemistry and physics were common requirements in the field. Mathematics is currently used in a number of fields in the same way.

(Piety requires me to remark that Comte was not himself a heavily biased reductionist. What appears to be that in the *Positive Philosophy* is only an installment of a story which is completed in the *Positive Polity*. In the story as a whole, there are three related "methods": "Objective," "Subjective," and "Synthetical." The first is atomic reductive; the second is what we shall describe as "rational"; the third is partly rational and partly a union of enquiry, morals, and social action into a single process.)

Molecular reduction. As in other cases, brothers in science are likely to quarrel more bitterly than cousins: those who espouse molecular reductive principles bitterly dislike atomic reduction. B. F. Skinner (1938) exemplifies the point, and in such wise that he provides an apt statement of the distinction between the two subspecies while nicely illustrating the affect which marks such choices. The object of Mr. Skinner's first aversion is a holistic principle.

> ". . . Behavior has that kind of complexity or intricacy which discourages simple description and in which magical[1] explanatory concepts flourish abundantly. Primitive systems of behavior first set the pattern by placing the behavior of man under the direction of entities beyond man himself. . . . In more advanced systems . . . , the ultimate direction and control have been assigned to entities placed within the organism and called psychic or mental. Nothing is gained by this strategem. . . . Some conceptions of the 'mind' and its faculties, and more recently the 'ego', 'super-ego', and 'id' are examples of inner agents or organisms, designed to account for behavior . . ."[2]

[1] Mr. Skinner has the distinction of being one of the first American scientists to use "magical" instead of "metaphysical" as a major epithet. However, the fact that epithets of this kind are not the exclusive property of the "hard fact" men is seen in the following: "During the 1920's and 1930's psychologists were, on the whole, rather adverse to theory. Governed by a naive metaphysical belief, they were apt to consider 'fact finding' the only task of 'scientific' psychology . . ." (Lewin, 1951).

[2] The attack on a holistic principle in this paragraph is obscured by simultaneous attack on anything without a "physical status."

He now moves in on atomic reduction:

"The important advance from this level of explanation that is made by turning to the nervous system as a controlling entity has unfortunately had a similar effect in discouraging a direct descriptive attack on behavior. The change is an advance because the new entity beyond behavior . . . has a definite physical status of its own. . . . Its chief function with regard to a science of behavior, however, is again to divert attention away from behavior as a subject matter. . . . The more sophisticated neurological views generally agree with the popular view in contending that behavior is itself incomprehensible but may be reduced to law if it can be shown to be controlled by an internal system. . . . Facts about behavior are not treated in their own right but are regarded as something to be explained or even explained away by the prior facts of the nervous system.

"Worst of all, it [physiological behaviorism] carried on the practice of seeking a solution for the problems of behavior elsewhere than in behavior itself. . . . The altogether too obvious alternative to a mental science was a neural science. The possibility of a directly descriptive science of behavior and its peculiar advantages have received little attention."

First, then, the demolition of holistic principles and nonphysical terms: "magical concepts," "strategems," "mind," " 'ego,' 'super-ego,' 'id'." This is followed by attack on atomic reductions (neurology and physiological behaviorism) and praise of treatment of the phenomena of behavior "in their own right." The latter phrase is the common presenting symptom of preference for molecular reductive principles.[3] Skinner then proceeds to bound his preferred subject-matter ("Behavior is what an organism is doing—having commerce with the outside world") with a further exhibition of his presenting symptom: "the peculiar properties which make behavior a unitary and unique subject matter."

He now thrusts at one form of anti-principled principle:

". . . We may proceed to a kind of description of behavior by giving a running account of a sample of behavior as it unfolds itself in some frame of reference. This is a typical method in natural history and is employed extensively in current work. . . . It may be described as narration. . . . From data obtained in this way it is possible to classify different kinds of behavior, it is not a science in the accepted sense. We need to go beyond mere[4] observation of a study of functional relationships. We need to establish laws . . ."

Skinner is now ready to describe and praise his own reductive molecule:

". . . It is presumably not possible to show that behavior as a whole is a function of the stimulating environment as a whole. A relation between terms as complex as these does not easily submit to analysis and may perhaps never be demonstrated. The environment enters into a description of behavior when it can be shown that a given part of behavior may be induced at will by a modification in part of the forces affecting the organism. Such a part of the environment . . . is traditionally called a *stimulus* and the correlated part of the behavior a *response*. Neither term may be defined as to its essential properties without the other. For the observed relation between them, I shall use the term *reflex*.

"The difference between the demonstration of a reflex and mere narration is . . . that no lawful relation . . . is asserted . . . The story is told simply of something that has once happened. The isolation of a reflex, on the other hand, is the demonstration of a predictable uniformity of behavior. In some form or other, it is an inevitable part of any science of behavior . . . Current objections to the reflex on the ground that in the analysis we destroy the very thing we are trying to understand, scarcely calls for an answer. We always analyze. It is only good sense to make the act explicit . . .

"So defined a reflex is not, of course, a theory. It is a fact. It is an analytical unit, which makes an investigation of behavior possible . . ."[5]

Molecular reduction is, then, the effort to find the irreducible minimum of the subject-matter under investigation. Among such paradigmatic molecules we have "the family" of one political sociology, the "cell" of nineteenth century general physiology and the "two-person group" of some recent sociology. In the case of B. F. Skinner,

[3] For a further example of this presenting symptom, see Durkheim (1950, Ch. I). It is interesting and touching that whereas most psychologists can now take physiology or leave it alone, a great many sociologists are still extremely tetchy about psychology. Thrice in a week I have heard young sociologists respond to a reductive remark by the phrase, "Well, if you *must* psychologize . . ."

[4] "Mere" rivals "metaphysical" as a favored epithet in defenses of form of principle.

[5] Scattered through these quotations are indications of a choice at Decision Point 3. We shall deal with this later.

the telling sentence is: "Neither term [stimulus, response] may be defined as to its essential properties without the other." That is, the reflex, constituted of stimulus and response, is asserted as the ultimate reduction *of behavior*. If what "stimulus" names is discussed apart from "response" (as it can be), the conversation is no longer about behavior but about something else—physical or meteorological or chemical phenomena. Similarly, "response" without "stimulus" would be physiology or biochemistry, not behavior, and, for that reason, unacceptable.[6]

From the point of view of enquiry as against enquirers, molecular reduction tends to shade off toward the atomic by invoking numerous *different* elements as determinative constituents. Pre-Mendelevian chemistry is a case in point; so also the general physiology which moved from "the" cell to three, five or ten kinds of cells (e.g., secretory, epithelial, nervous, muscular, germ). A similar psychology would arise from an attempt to discriminate classes of reflexes and to account for complex behaviors as combinations. Even in such cases, however, the personal loyalty of the enquirer appears to attach to the molecular character of the simple: that it is a simple which is peculiarly chemical and not physical; or social and not psychological; or behavioral and not neural.

The fact that molecular reductions are incomplete principles of enquiry lends credence to this hypothesis. Unlike atomic reduction to a field of enquiry which has terms already laid down, the molecular reduction requires further invention. Given the stimulus-response reflex, for example, the enquirer must still decide what to observe or measure about that molecule and how to interpret his data. For this purpose he must append additional factors and these may be taken from one of the other forms of principle: holistic, rational, anti-principles—even atomic reductive. In brief, molecular reduction often serves only to dignify the science by asserting the existence of a unique subject-matter requiring its service.

". . . Practically all sociologists now demand a separate existence for their science; but . . . the common-sense view still holds that sociology is a superstructure built upon the substratum of the individual consciousness. . . .

"What is so readily judged inadmissible in the matter of social facts is freely admitted in the other realms of nature. Whenever certain elements combine and thereby produce, by the fact of their combination, new phenomena, it is plain that these new phenomena reside not in the original elements but in the totality formed by their union. The living cell contains nothing but mineral particles, as society contains nothing but individuals. Yet it is patently impossible for the phenomena characteristic of life to reside in the atoms of hydrogen, oxygen, carbon, and nitrogen. . ." (Durkheim, 1950, p. xlvii).

And from the psychiatrist Maslow:

"This conclusion exposes the essentially theoretical nature of the entire reductive effort . . . It is simply a reflection or implication in science of an atomistic, mechanical world view that we now have good reason to doubt." (Maslow, 1954).

Holistic principles

Holistic principles are most conspicuous in the frankly taxonomic sciences—zoology, botany, minerology—and in physiology from William Harvey to recent times. Holistic principles require an account of the subject-matter of interest in terms of the combination of qualities or constituents which, as organized, sets that subject-matter apart from all others. Parts or constituents are used, as in atomic reductive principles, but the properties of the constituent factors do not provide an adequate or complete account of the whole. *That* is sought in the whole itself, as well as in the parts. Indeed, some properties of the constituents are sometimes treated as conferred by their place in the organization of which they are parts. For this reason, such principles are sometimes called "organic." The original Aristotelian prescription of such principles is still viable:

[6] The phrase "dyadic group" frequently replaces my "two-person group," not only because it sounds impressive but because it better asserts the "right" unit; i.e., a dyadic group is *a* something with two parts, not two somethings interacting. It is a social molecule, not psychological atoms—as Skinner's "reflex" is a psychological molecule whose wholeness is not to be divided.

". . . the organizing principle is the starting point in natural things as it is in those things which are made by man. Consider how the physician or the builder sets about his work. He starts by setting for himself a definite picture, the physician, of health, the builder, of a house. This picture he uses as the guide and explanation of each subsequent step that he takes. . . . In the works of nature, the organizing principle is even more dominant than in works of art such as these. . . .

". . . there is also a factor manifested which may be called contingent necessity. For if a house or other such object is to be realized, it is necessary that such and such material shall exist; and it is necessary that first this and then that shall be produced—and so on. . . . So also is it with the production of nature. The starting point for living things is that which is to be. But this, being of such-and-such characters necessitates the previous existence or production of this and that antecedent . . .

"The fittest mode, then, of treatment is to say, a man has such and such parts because the organization of man includes their presence and because they are necessary conditions of his existence." (Paraphrased from *Parts of Animals*, Bk. I, Chapter I)

And from Maslow (1954):

"One essential characteristic of holistic analysis of the personality . . . is that there be a preliminary study or understanding of the total organism, and that we then proceed to study the role that our part of the whole plays in the organization and dynamics of the total organism."

Formal - material holistic principles. Formal-material holistic principles follow the Aristotelian prescription in assigning roles to both the material constituents and to the organized whole. For example,

"The atom, we now find, is . . . a mechanism or organism in which the properties of the whole depend not *solely* on the nature of its parts but *also* on the peculiar relatedness of its parts; while an organic molecule . . . is, again a mechanism or organism in which the properties of the whole depend upon the relatedness of its parts as well as upon their nature. Now for *descriptive* purposes, and for visualization, the parts of the atom or molecule must be in our minds; and we cannot therefore dispense with the results of analysis. Yet in spite of such knowledge of the parts certain fundamental properties of the *whole* will elude us in each case unless we study it while intact. We can only properly define the whole in terms of its activities. . . . In all cases structure, internal events and functions must be thought of as inseparable if we are to grasp the essential nature of such systems . . ." (Needham & Baldwin, 1949, pp. 179–190).

In a less dramatic form, such a principle is seen in a physiology which first assigns certain necessary activities to the organism (e.g., ingestion, digestion, excretion, reproduction, etc.) and then describes each organ, tissue and enzyme-system by reference to what it contributes to one or another of these defining activities and how it does so.

Formal holistic principles. When the distinguishing character of the subject of interest is treated as capable of embodiment in any one of a variety of materials or sets of parts, the holistic principle becomes formal. Enquiry guided by such a view may use material parts as evidence of the character of the whole since they are taken as effects or requisites of it but the stable object of enquiry is the pattern, organization or form exhibited *via* the material. For example:

"Democracy—like forms of group living—cannot be defined adequately by isolated elements of conduct, rules or institutions; it is the larger *pattern* of group life and the group atmosphere which determines how society is to be classified." (Lewin, 1954).

and from Maslow (1954) again,

"Since these specificities have the same source or function or aim, they are interchangeable and may actually be thought to be psychological synonyms of one another . . ."

Such enquiries entail a search for a variety of embodiments. One set of material parts, being only one means of expression, may, as stubborn matter, either fail to express the full pattern or mix its own properties with it. Hence, the nature of culture is sought in cross-cultural study. The nature of the organism is sought through numerous experiments in which parts are altered and the coordinate changes in all other parts are scrutinized in order to determine the stable pattern which is reconstituted by all such sets of changes (J. S. Haldane, 1932).

Needless to say, the exponents of such principles, dubbed mystics, do as they are done by, especially to atomic reductionists; e.g.,

". . . the orthodox credulity with which so many writers continue to refer to the 'mecha-

nisms' of various living activities is to my mind pathetic in its blind clinging to what has come to be regarded as a sacred scientific authority." (Haldane, 1932)

It is worth noting that holistic principles share with molecular reductions an emphasis on the apartness of the subject of interest. It must be studied "in its own terms." I suspect, however, that a factor analytic study would show that scientists' adhesion to holistic principles is heavily loaded with a passion for wholeness or something of which wholeness is symbolic rather than with a need to create an independent domain of investigation.

Rational principles[7]

If atomic reduction seeks its likely story in "downward" terms, while moleculars and holists try for a statement of the subject in its own terms, there is a remaining malapropic possibility: reduction upward. Principles of this kind require that the subject of interest be seen as given its character by its place in some larger determinative whole or by some *ratio* imposed from without. Such a principle is seen in one of its simplest guises in socially dominated theories of personality in which the human organism is treated as a nearly-blank slate on which the society writ small is what is mistaken for a person.

Most such principles have a distinct Platonic, Cartesian, or Deistic cast. That is, the process of determination is rarely referred to a *vera causa* or to specified processes, as introjection and identification are used by some of the conservative social personologies. Rather, the determiner is seen as some sort of rational structure of relations with no particular *relata* (a "configuration"), either subsisting in its own right or as the defining part of the material, determinative surround. The Keplerian conic sections, the Relativity field (insofar as it is not the product of objects in the field), and Lewin's psychological field (in part), are cases in point.

Einstein (1936) succinctly describes this rationality as follows:

> "When I speak here of 'comprehensibility' the expression is used at first only in its minimal signification. It signifies that by the creation of universal concepts and relations among these concepts, and also by stable relations established somehow or other between concepts and sense-experience, the latter are brought into some sort of order. . . .
>
> "All that is essential is the attempt to present the manifold of concepts and propositions which are close to experience as propositions logically derivable from a basis—as narrow as possible—of fundamental relations which can in themselves be freely chosen (axioms). . . ."

By "a Platonic, Cartesian or Deistic cast," I mean to suggest a common quality of the enquiring principles but not to suggest that idealist metaphysical views are necessarily espoused. What justification will be used in defense of such principles is hardly predictable beyond the fact that very frequently they will be asserted to be "best" or "true" or "most widely accepted" science. Clark Hull's book, *Principles of Behavior* (1943), is a case in point. Here, amid echoes of J. S. Mill, William Whewall and more recent logicians, deductive systems (involving primary terms which are hardly Keplerian or Einsteinian) are defended partly on the ground that if empirical generalizations can be incorporated into a deductive system, *post facto*, their credibility is somehow enhanced.

It should be noted that rationally instituted theories are not to be taken as identical with bodies of theory presented as deductive systems. The mark of a rational system is not in its form but in its content. It consists of assignment of causal efficacy "upward," whether to the configuration of a material "whole" of which the subject of interest is a "part" or to a Platonic-Cartesian *ratio*. Though some deductive systems are fruits of enquiry pursued in this way, some are not. The deductive structure may be merely imposed on the results of enquiries instituted by other principles, e.g., the

[7] A word about the word, "rational." In this context it does not mean—honorifically—logical, reasonable or sane, thus suggesting that other principles are unreasonable, "merely empirical" or insane. Nor does it mean, as does the pejorative "rationalistic," the elaboration of "self-evident" premises into conclusions to be accepted without recourse to empirical data. What it does mean, the text will try to show. My apology for the misleading word is that I can find no better one.

Clark Hull material cited above. (See also the section on Imposition of Deductive Structures). On the other hand, a theory developed in terms of a rational principle may be presented apart from a formal, deductive system.

A further difficulty exists in the distinction of rational from formal-holistic principles since the latter appeal to a *ratio* as do some of the former. The distinction rests on the fact that the *ratio* of formal-holistic principles is something peculiar to the whole investigated while the *ratio* of rational schemes is a form whose variants or subspecies reign over greater reaches of investigable phenomena. Thus the normal curve, other probability functions, exponentials and other algebraic functions are characteristic *ratios* of rational schemes since they may be applied indifferently to the investigation of any subject-matter or all subject-matters.

The fact that ratios are characteristic of formal-holistic schemes as well as of rationals also complicates somewhat the question of who prefers what among principles. Two pairs of factors intersect one another in this case. On the one hand, there is the choice between universal and local subject-matters. On the other hand, there may also be a tendency to prefer *ratios* as such or to reject them as such. Thus four classes are generated, only three of which are readily identifiable. A preference for *ratios* and for a local subject-matter is satisfied by formal-holistic operation. Preference for *ratios* and for a universal subject-matter yields rational schemes. Rejection of *ratios* and preference for local subject-matter can be satisfied with formal-material holistics. But rejection of *ratios* and preference for a universalized subject-matter constitute an extremely obscure class. These may be the men who practice holistic enquiries but persistently preach vague or arbitrary sermons about the need for synthesis, integration, or unity of the sciences.

By contrast to such ordered structures as those of Einstein, von Neumann and others, an entirely *ad hoc* example is seen in the following:

> **". . . Thus in general, the spatio-temporal pattern of activity would be determined . . . by three factors: (i) the micro-structure of the neural net and its functional properties; (ii) the afferent input; (iii) the postulated 'field of extraneous influence'. . .**
>
> **"It can be claimed that no physical instrument would bear comparison with the postulated performance of the active cerebral cortex as a detector of minute 'fields of influence' spread over a microscopic pattern and with temporal sequences of milliseconds. The integration, within a few milliseconds, of 'influence' picked up at hundreds of thousands of nodes would be unique, particularly when it is remembered that the integration is no mere addition, but is exerted to modify in some specific way 'a shifting harmony of sub-patterns' of neuronal activity, achieving expression through the modifications so produced.**
>
> **"Thus, the neurophysiological hypothesis, is that the 'will' modifies the spatio-temporal activity of the neuronal network by exerting spatio-temporal 'fields of influence' that become effective through this unique detector function of the active cerebral cortex. . ." (Eccles, 1953, p. 277).**

Anti-principles

The most conspicuous of anti-principles arise from and embody the familiar view that science ought to avoid principles all together (or may avoid them without loss) and be content to report the facts. What properly constitutes "the" facts then becomes the covert issue of principle. The schism on this score creates two large departments.

A third department arises from modern vestiges of the view that some things of this world are not lawful—or are otherwise insusceptible of scientific enquiry.

"Laws of nature". On the one hand, "the facts" take the form of "laws"—algebraic or verbal equations whose terms are alleged to be in one-to-one correspondence with sense-experienced and "objectively" discrete aspects of the subject-matter. Thus, "the facts" are the sensible covariances of measurable parameters. It should be emphasized that the mathematics involved in such enquiries is treated as a system of notation or measurement. There is abhorrence of the rationalist notion that mathematical functions express rational structures to which the world might be expected to conform. Indeed, anti-principle equations might well run to seven or eight powers were it not for the relative ease with which

one parameter can usually be split into two or three when the number of inflection points grows too big for comfort. Mach's familiar strictures on Galileo constitute a case in point:

> "In Galileo's conception of the universe, the *formal mathematical* and *aesthetical* point of view predominates. . . . The planetary system had not yet taken the shape of a *genuine physical problem* for him." (Mach, 1942) (Italics mine).

This theme is addressed to Newton with even greater violence by Karl Pearson:

> "I have accordingly expressed my own views on the subject; these may be concisely described as a strong desire to see the terms matter and force, together with the ideas associated with them, entirely removed from scientific terminology, to reduce, in fact, all dynamics to kinematics." (Clifford, 1946, p. xiv).

"Causes". Elsewhere one finds "the facts" defined as antecedent-consequent "causation." This way of enquiring differs in no important wise from the J. S. Millsian formula. The world is seen as a web of partial uniformities whose separate strands —of invariant antecedent-consequent relations—are the only proper objects of enquiry. The webbing, itself, the frequent or invariant recurrence of N antecedent-consequent strands to constitute, e.g., a man or a swan, is treated as a locus of problems but not *as* a problem for enquiry.

For the purposes of an etho-psychological study of the scientist, it is useful to note that the causal anti-principle is Millsian also in the fact that it invites the "method of differences" in all its compact neatness as the basic experimental design. Indeed, both forms of anti-principles correspond to the textbook ideal of "the controlled experiment."

Adherence to this anti-principle in opposition to concern for the webbing or whole as constituting the central problem of enquiry lies at the base of much of the perennial strife over "teleology" in the biological sciences. It is rarely the case that "teleology" is, in fact, espousal of a notion of a conscious "wisdom of the body" or a similar conscious wisdom on the part of birds or earthworms. Partisan attack will often so describe it, but the researches attacked usually disclose only that the researcher has embraced a material-formal holism. He treats the distinguishable parts and processes of the organism as matters fully understood only when interrelated, as constituting a whole, the organism. In Aristotelian terms, formal and final causes are identified: that which is the final cause and that which is the formal cause are taken as the same—the mature organism; and an account of efficient and material causes is given in terms of the role or function of, the service performed by, each efficient cause relative to the whole going concern. It is this account which is deemed "unscientific" by adherents to causal anti-principles. [In animal behavior studies (e.g., E. S. Russell) the opposition is made more poignant by translation of the "going" concern into one or several massive *hormes*, as food, nesting, mating, etc.]

In attempting to identify studies guided by the causal anti-principle, it is important that it be not confused with concern for described or narrated facts in general. Publications consisting of "pure" description or narration are often preludes to studies whose controlling notion is quite otherwise. The most "teleological" of embryological investigations may involve numerous publications of plates, drawings and verbal descriptions devoid of coherent interpretation in those particular papers. The same holds for accounts of animal behavior, individual and group human behavior, physiological and biochemical studies, which may be holistic, atomic reductive, or even rational in their completed form.

Finally, it should be noted that biologists and psychologists adherent to these anti-principles have an extremely hard time being faithful. The word and the notion of functon creep in time and again; so does "teleology" in the guise of "drive," "anticipation," "adaptive value," "operant behaviors." I suspect that the anti-principled scientist's losing struggle to defend his virtue is due to the exigent demands of the organism as a subject-in-fact.

Particularities. One or another plausible distinction between what is matter for enquiry and what is insusceptible to scientific

investigation have been commonplaces of "philosophies of science" since Aristotle. The line was early drawn between non-living material on the one hand, and life, human life, art and morals on the other, all the latter being distinguished as "free" in some sense where matter was "fated" or obedient to law.

This location of the dividing line persisted through almost the end of the nineteenth century. Physicians spoke up from the floor of Medical Society meetings to reject general statements about organisms and about disease as, *qua* general, contrary to fact. (Even Claude Bernard, apostle of positive science, was constrained at one point to describe the organism as held in lawful bounds only by the constraints of the physical environment.) And, of course, the free will of man was interpreted by many as placing human behavior, and especially the behavior of valuing, far beyond the limits of the investigable.

It is often said that modern scientists make no such distinction, on the ground that the only persuasive demonstration that some class of matters is incapable of scientific investigation is the durable failure of efforts to investigate them. This is not the universal view, however. The line has been moved up but not off. Sporadic but not rare expressions of scientists now place it so as to mark off particular events, and especially the particular human event (whether social or individual) as beyond the scope of scientific enquiry, however effective enquiry may be in dealing with *classes* of events.

Some truth lies back of this, of course. The scope of an enquiry rarely includes the full richness of its subject-matter and what is omitted from its formulations is most vividly seen in the individual case which acts "contrary to the scientific law" because what is acting is something more complex than the subject-of-enquiry subsumed in the "law" in question. It is also the case, however, that, quickly or slowly, enquiries expand their purview to include more and more of the details which constitute their subjects and there is no substantial reason to suppose that this expansion has limits (sub-atomic particles excepted) prior to the limits of the subject-matter itself. We shall further discuss this aspect of enquiry later, under the heading of "Validity." Meanwhile, we must leave a place among the ranks of the "anti-principled" for the view that science should not spend its substance on certain problems because they are incapable of solution. A case in point is Franz Boas:

> ". . . it seems justifiable to question whether any generalized conclusions may be expected that will be applicable everywhere and that will reduce the data of anthropology to a formula which may be applied to every case, explaining its past and predicting its future.
>
> "I believe that it would be idle to entertain such hopes. The phenomena of our science are so individualized, so exposed to outer accident that no set of laws could explain them. It is as in any other science dealing with the actual world surrounding us. For each individual case we can arrive at an understanding of its determination by inner and outer forces, but we cannot explain its individuality in the form of laws. . . .
>
> "In short, the material of anthropology is such that it needs must be a historical science, one of the sciences the interest of which centers in the attempt to understand the individual phenomena rather than in the establishment of general laws which, on account of the complexity of the material, will be necessarily vague and, we might almost say, so self-evident that they are of little help to a real understanding." (Boas, 1955, pp. 257–8).

Primitive principles

John Dewey pointed out that one of the origins of knowledge of subject-matters (scientific knowledge) is in the effort to codify and extend knowledge of ways and means (common-sense knowledge). This partial dependence of science on common sense is usually conceived as expressed in only two contexts: historically, in the sense that the origin of science as an enterprise is thus explained; and sociologically, in the sense that social approval and disapproval of scientific enquiries via the status accorded, financial support afforded or withheld and so on, go far toward determining what enquiries proceed and which are retarded or stopped.

In point of fact, however, there is a third expression—an ontogenetic repetition of phylogeny. Immature sciences and sciences in moments of frustration and regression

often refresh their enquiries by renewed contact with the earth of common sense. Conceptual frames which never were or which seem to be exhausted are replaced by numerous *ad hoc* investigations framed in the terms of the queries which would normally speak to practical problems.

For example, one current investigation of cancer discards all conceptions of the microchemistry and biology of the cell in favor of *ad hoc* tests of the effects of an array of chemicals chosen almost at random. Political scientists ask such question as, "What filiations and adhesions are correlated with the voting behavior of such-and-such group?"; "How did Smith reach his present position of power?"; "Who actually controls such-and-such?". Thus scientists ape the questions men ask in order to plan a political campaign, a personal career or other efforts to get one's way. Studies of small groups in terms of what will increase the efficiency of workers and their loyalty to management is another case in point. So also some studies of learning in terms of the enhancement of scholastic achievement.

On "models" and "model-building"

"Model" and "model building" are currently words with which to conjure, though not yet, like God and Game Theory, spelled with upper case initials. Their referents add little to the scheme of principles so far described but require comment nonetheless since they represent emphases which bestow some advantages on enquiries under certain circumstances.

A heterogeneity of four activities and devices are found labeled as models by enquirers. Two of them instance or approximate the use of rational principles; one represents the attempt to incorporate the fruits of an enquiry into deductive structures; the fourth is simply the self-conscious use of plausible analogies.

Rational models. Full and explicit use of rational structures as "models" is exemplified by the attempt to relate any large scale mathematical structure, such as Game Theory or a probabilistic calculus, to a body of phenomena. Thus, the activity is pretty much the same as Einstein's application of Reimannian geometry to space, including the premises underlying that application, the operations required, and the fruits which accrue.

The premise concerns the mathematically ordered or orderable structure of the phenomena in question. Either the world is ordered in this way or it is heuristically valuable to constrain those portions of it which will so submit to the structure of a logical scheme.

The operations involved are simply describable but extremely difficult in practice. Factors are sought in the subject-matter which can be assigned as the referents of some of the terms of the mathematical structure. By this means, the theorems (and corollaries thereof) of the mathematical structure are transformed into existential assertions. The truth or falsehood of these existential assertions are then tested. If the tests reveal that the effort is entirely successful, we now have a demonstrated theory of the rational kind. If the effort is partially successful, either of two alternative procedures may ensue. On the one hand, there can be an effort to discriminate that aspect of the subject-matter which conforms to the rational structure, to distinguish it by definition from that which does not conform, and to retain the fitted and its fitting structure while relegating the remainder of the subject-matter to an agenda of future enquiry. On the other hand, there may be an effort to modify the rational structure itself so as to adjust it to the subject of interest. (The choice between these two procedures is involved in decision points 4 and 5 to be described later). If the effort is a failure, the investigator may try some other rational structure.

A successful effort of this kind has precisely the value which an investigator may assign to rational structures. If he prefers them above all others, the effort constitutes a great new synthesis. If he does not approve, he will demand that the rationalist investigator ocularly demonstrate the existence in fact of measurable entities corresponding to all the terms of the rational scheme. In either case, however, it is desirable to note that the use of a ready-made mathematical structure has the advantage,

if it works, of presenting by a single *coup* a great number of inferred consequences of the implied hypothesis.

Miniature use of rational models is seen in efforts to fit some part or aspect of known data to a standard form of algebraic or differential equation, or other small-scale scheme of related symbols. This activity is usually undertaken (e.g., bio-mathematics) with the same valuation in mind as in the full-scale case. If the effort is successful it is usually followed by the second alternative described above. That is, the investigator, having achieved a mathematical first approximation to known data, now attempts a more adequate approximation.

Imposition of deductive structures. Modeling of a different kind can be invoked near the termination of a course of enquiry. A subject-matter having been defined and a few general terms discriminated, numbers of pairs, triplets or slightly larger sets of more specific terms will be devised independently of one another and the entailed enquiries of each set pursued independently.

The aggregate of these several researches may then be brought together and the services of a logician or analyst requested. His task is to determine whether the compendium of terms, "laws," conclusions, etc., is capable of rearrangement in the form of a deductive system and to develop such a system if it is possible. (E.g., Clark Hull.)

The advantages which may accrue to such an undertaking are three. First, a specialized training is invoked which may be better able to detect ambiguity or vagueness in the terms of the enquiry than was the original investigator. Second, incoherence among the terms or contradictions among their containing propositions are likely to be identified. Third, inferences may be derived whose purports imply new experiments which might not otherwise have been envisaged. Needless to say, these outcomes are valuable. Whether a logician is likely to be inclined to undertake such a task after the fact of conception and ensuing enquiry is another matter. His position is almost as bad as that of the statistician whose help is sought only after the experiments are done and the data collected.

Self-conscious trials of analogies. When new principles are required in a field of enquiry, they are usually sought from two sources. They may be borrowed from another field (preferably a "neighboring" one). They may be devised by a variant specification of a general and common idea, such as that of "particle." Both sources are common. Both are entirely legitimate.

In the past few years, use of the first of these sources has been dignified by calling it model-making or -seeking. Thus a physiologist proposes that a society or culture be treated as if it were an organism. A social scientist may try to examine an area in which two peoples have come into extensive contact by asking whether the biological phenomenon of hybrid vigor, "heterosis," has a social analogue. Neuropsychological investigations are instigated in terms of computing machines and their functions, and so on.

The advantages of such a procedure are the advantages which accrue from seeking and finding any new terms of enquiry—when they are needed—whatever we may call the search. There is even an advantage in pursuing the search self-consciously: we look systematically for new and likely terms. There is also a disadvantage: a pretentious new name for the process may cast such an aura of prestige that the search for new terms is pursued when new terms are neither necessary nor desirable.

CRITERIA FOR THE JUDGEMENT OF PRINCIPLE (DECISION POINT 2)

Principles for enquiry are not, of course, accepted out of hand by the scientific community. Nor are they usually private inventions privately used. On the contrary, they are required to be presented to the community where they are scrutinized, debated, and then accepted or rejected for the more searching and expensive test which consists of their use in enquiry.

The arraignment of principles for debate is commonly instigated from one of two sources. On the one hand, no principle may have proved of extensive and durable worth in the field thus far. In such a case, numerous principles and numerous proponents of principles will compete for the attention and approval of the participating community.

This situation is, so far, continual in the *methodenstreit* disciplines: psychiatry, psychology, sociology, political science. One hesitates to assign a number to the variety of conceptions of the person, of learning, of society and of political action now in use.

In other fields, such crises are recurrent rather than continual. New phenomena are discovered; the effectiveness of an old set of principles has been exhausted; a current set has been effective enough to reveal complexities in the subject-matter which the revealing principle cannot encompass. In such crises, new principles are proposed or, more often, revisions and emendations of existing principles.

Thus, in 1919, E. M. East meets an alleged inadequacy of the Mendelian conception of the apparatus of heredity. Biologists assert that the Mendelian scheme must be discarded since it cannot take account of characteristics of a species which exhibit continuous variability. East responds by exhibiting the genic apparatus as a mechanical model of the expansion of the binomial and the normal curve as the limit of such an expansion. In two beautifully designed papers, he then shows that these notions, together with a few corollaries, enlarge the then-current view of the gene mechanism so that it can encompass "blending" characters as well as those which fall into a few discontinuous classes. More recently, inadequacies of the notion of dominance-recessiveness in genetics are remedied by the added notion of penetrance. The Lorentz note on the Michelson-Morley experiments is a more drastic case in point.

The documents which propose principles or their revision and the critical statements which the proposals generate show the existence of four major criteria by which judgments are made.

The four criteria are: interconnectivity, adequacy, feasibility, and continuity. Interconnectivity concerns the extensive domain of subject-matters subsumed by the proposed principle. Adequacy concerns its intensive domain, the degree of complexity or "completeness" with which the principle subsumes the details of the subject-matter. Feasibility concerns the ease, economy, precision, and consistency with which the data required by the principle can be collected. Continuity concerns the ease or difficulty with which the new principle can be made to contain the bodies of knowledge previously formulated in other sets of terms (i.e., by translation or by subsumption as a special case).

The referent qualities named by these criteria are not exclusive of one another nor are they often treated as if they were. Hence, at this decision point, unlike the first, preference is not for one as *against* the others nor is preference usually shown by attack on other criteria. Instead, choice is exhibited as a rank-ordering or weighting of the four. In proposing statements, weighting is seen in what the author presents as the advantages to future enquiry of his proposed principle and in those possible effects on the ensuing enquiry on which he is silent or brief. Among critical statements, the weighting is seen in the character of the shortcomings which are selected for attack and in the advantages which are alleged as reasons for acceptance.

Interconnectivity

Emphasis on interconnectivity is seen clearly and briefly in the following:

> "Not infrequently it has been stated that theories which merely explain known facts are of no particular value. I cannot agree with this view. Particularly if the theory combines into one logical system known facts which previously had to be treated by separate theories; it would have a definite advantage as an organizational device. . . ." (Lewin, 1951).

A major emphasis on interconnectivity is seen in Einstein's argument for field theory as against classical mechanics and suffices to exhibit the character of this criterion:

> "In spite of the fact that today we know for certain that classical mechanics breaks down as a ruling basis for the whole of physics, it still stands in the center of physical thought. The reason for this lies in the fact that . . . we have not yet arrived at a new basis for physics *from which we can be certain that the entire manifold of investigated phenomena and successful special theoretical systems can be logically deduced.* . . .
>
> "To the attempt to make mechanics fundamental to the whole of physics, light and electricity present insurmountable obstacles. This leads to a field theory of electricity and beyond this to the attempt to base the *whole of physics*

on the concept of field. . . . (Einstein, 1936) (Italics mine).

A high ranking of interconnectivity over other criteria appears to stem from three (possibly four) sources which may be psychologically independent. These sources are, respectively, a sensitivity to subject-matters, a sensitivity to processes of enquiry, and what appears to be a generalized Oceanic state.

The first is seen in pleas for interconnectivity ("interdisciplinary approaches") which argue, for example, for a psycho-social economics because of the influence of status structure, wants, etc., on the market; or for certain dynamic theories of personality as against others because of the need adequately to recognize the roles of culture and society on personality formation; or for a psychological political science for similar reasons. Arguments responsive to subjects come frequently from persons who give some of their time to the executive foci of the investigative disciplines: engineers, statesmen, and physicians. This is to be expected, for in these professions men encounter subject-matters in a more nearly pristine state. The constituent matters of practical problems rarely conform to the boundary lines which insulate them from one another in the milieu of enquiry.

Similar arguments which are not quite the same are seen in pleadings which carry the Durkheimian need for a self-sufficient science with a peculiar subject-matter one step further; they speak for principles which will unite the subject-matters of several sciences under the aegis of one of them as queen of the sciences. Claude Bernard's notion of the proper dominance of physiology over anatomy, pathology, and medicine is a case in point. (*An Introduction to the Study of Experimental Medicine.*)

The argument from commitments about the character of enquiry is exemplified by Ludwig von Bertalanffy:

> "Modern science is characterized by the trend toward continually increasing specialization. This development is unavoidable in view of the ever-increasing amount of facts. . . . It is generally acknowledged, however, that this specialization entails grave dangers for the development of science. . . .
>
> "On the other hand, the modern development in the various sciences shows a remarkable parallelism in basic viewpoints and constructs. This parallelism . . . indicates a hitherto unsuspected unity of the modern world-picture, and a uniformity of isomorphy of constructs and laws in different fields.
>
> "In view of these facts, the present author has proposed a new basic [sic] discipline, called General System Theory, which aims at establishing those principles which are common to, and thus applicable in various fields of science from physics to biology to psychology or to the social sciences."[8] (Undated Mimeo)

In recent years, such arguments are commoner among philosophers, especially logicalists, than among working scientists.

The Oceanic plea for interconnectivity seems to depend for evocation on no specific occasion in either the relations of subject-matters or of enquiries. The argument is of the form: The World is One; therefore, Science should be One. And unity is sought as much for literary criticism and biology as for, say, psychology and neurology. The argument is indigenous to history but occasionally seen in other disciplines.

A class intermediate between the Oceanic and the subject-matter sensitive is represented by such items as a plea by a psychologist to base literary criticism on psychological principles; by a physiologist to base ethics and politics on the behavior of cells and tissues; by sociologists to assimilate history to sociology. I have tended to treat these as vulgarities irrelevant to enquiry.

This may be an error. The poignancy or pointlessness to *us* of a given plea for interconnection depends on the extent to which *we* see the subjects involved as related or irrelevant. This, in turn, depends, not on the facts objectively given by the subject-matters, but upon the state of enquiry at the time. That is, related or "neighboring" fields are those which, literally, *have been related by enquiry*. They are the fields between which the terms of enquiry (or the terms for solving practical problems) have already managed to establish a few connections. And "irrelevant" subject-matters are

[8] Similar comments may be found in papers by von Bertalanffy in *The British Journal for the Philosophy of Science*, 1950; *Human Biology*, December, 1951.

those for which connective terms have yet to be devised. Hence, the Oceanic pleader or the pleader for connection between such "remote" areas as ethics and physiology may well be a man who sees farther into a possible future than we do.

This point also bears on the problem of estimating the ranking of interconnectivity relative to other criteria in a given science at a given time. For, where some connective principles between the field in question and another do not exist, the scientists in that field who put a high value on interconnection lack channels for exhibiting their preference. Thus, a good judgment of the ranking of interconnectivity as a criterion must be made by reference to the actual interconnections which exist at the time the enquirer is exhibiting his argued preferences among principles.

The criterion of adequacy

Where the interconnective potency of a principle bears on the relations established among subject-matters the adequacy of a principle refers to the connections it makes *within* the immediate subject-matter, to the "completeness" or complexity with which it identifies components, factors, variables, and promises to establish satisfactorily complex connections among them. Thus Clark Hull (1932, p. 43) praises the "goal-gradient hypothesis":

> "It is hardly accidental that a single principle should be able to . . . integrate such diverse phenomena as the choice of the shorter path, Weber's Law, the elimination of blind alleys, the order of their elimination, the rate of locomotion through the different portions of a maze, the short-circuiting of symbolic-act sequences, and the origin of ideomotor action."

So, similarly, does Koffka (1922, p. 544) argue for the notion of "figure";

> "Very good, you may say, but what is the advantage of this new way of describing simple experience? It seems on the face of it so much more complicated, so much less systematic, than the old way. This, indeed, is a fundamental question, but it cannot be answered by arguments —only by facts. It must be shown that in all fields as well as in the field of choice-training (Wahldressuren) this new description explains the facts of experience more easily and better than they can be explained by the traditional view."

Enquirers see a need for more "adequate" principles in three sources. First, it is seen in the subject-matter as currently known through existing principles: e.g., that the notion of independent "traits" is inadequate to the interactive character of problem-solving competence; that mass and charge are inadequate to represent the differences among fundamental particles, and so on.

Second, it is seen in what is *not* known about the subject-matter, i.e., in problems formulable in the currently orthodox terms but not solvable therein, as when, for example, "contradictory" or incoherent results are obtained. Such an inadequacy occurs in a physiology pursued in terms of single-function organs which discovers an organ with two functions, or in the "paradox" of an *a* judged equal to *b* and a *b* judged equal to *c* when *a* is judged greater than *c* (e.g., Stumpf's paradox.)

Third, and much more radically, it may be seen in the possibility of posing new problems and addressing novel questions to a subject-matter: the Lorentz suggestion of dependency between velocity and length; studies of emotional determinants of susceptibility to infection; the Haldaneian effort to ask what changes in a supposedly stable anatomy and biochemistry can be brought about as adjustments to radical and persistent environmental changes; efforts to investigate alteration of a supposedly stable intelligence quotient.

Among these three sources there is a suggestion of a conservatism-freedom scale, the first being most conservative of existing principles. This property may be relevant in an investigating scientist's prejudices.

It should be remarked that discrimination of adequacy and interconnectivity is relative to the state of enquiry in much the same way as is the notion of "relevant" or "neighboring" fields. Whether the failure of an existing principle is a failure in interconnectivity or in adequacy is a question of whether elements left unrelated by that principle are understood as parts of "one" subject-matter or of "two," and this is a function of the principle or principles operative at that time. Thus, at one point in the psychological literature, "individual behavior" and "social behavior" are spoken

of as independent fields requiring some measure of interconnection. Later, similar matters are named as aspects of a single "subject" whose mutual dependencies require more thoroughgoing investigation. Adequcy and interconnection require discrimination, nevertheless, for debate concerning competitive principles does, in fact, occasionally turn on the relative weighting of these two criteria.

The criterion of feasibility

Little need be added to the remark that feasibility concerns the ease, cost, precision and reliability with which the data required by a principle can be collected. In many cases, this is only a matter of pointing to ways in which familiar techniques and instruments can be adapted to the new purpose. In other cases, unfamiliar forms of data, new experimental patterns, or new devices are required. It is in the second kind of case that differences in weighting of feasibility as against other criteria become most visible. On the one hand, high cost in time or money, the unproved accuracy of new devices, and so on are given as reasons for rejection of the principle. On the other hand, the defending or proposing document may consist in large part of efforts to disclaim such handicaps or, better, to remove them by reporting success in overcoming them.

The papers of E. M. East, mentioned earlier, constitute a case in point. It is a curious commentary on the biologists of the time that what East proposes is not new. The conceptions themselves and the greater adequacy they would confer on enquiry into genetics are visible even in Mendel's original paper. Still other papers have already reasserted the same points. The major contribution of East is to exhibit the feasibility of the conceptions, or, rather, to make them feasible by explaining precisely where and how to use them. He explains the significance of certain parameters of a frequency distribution as data of inheritance. He explains what new kinds of crosses and breeding patterns are required, why they are significant and how the resulting data are to be interpreted. This is done, not by a pure exploration of the conception, its corollaries and their consequences but in the context of two full-scale researches which are nothing more nor less than exhibitions of the fact that the data required by the conceptions can be collected and interpreted. In effect, these papers exhibit the weight put by East's audience on feasibility, or, conversely, the weight which East puts on adequacy. (Here, as in the case of the sources of inadequacy, a conservative-free factor is suggested. East is ready to abandon or change current notions of the meaning of breeding data, and current experimental patterns and learn a little statistics; his audience is reluctant.)

The criterion of continuity

This criterion brings into even sharper relief the role of a conservative-free factor operative in the weighting of criteria. The notion of continuity concerns the extent to which new principles arise from or can be connected with extant principles; and, consequently, how readily the knowledge accumulated in terms of older principles can be assimilated to the knowledge which may arise in terms of the new. Insofar as the new principle departs radically from the old, there is scope for the play of a conservative-free factor. For the advantages of a new principle lie in future enquiry and new knowledge and these advantages, in the case of principles which are radical departures, are costly as far as existent knowledge is concerned. The old knowledge may require extensive reformulation. Extensive re-education and sweeping rewriting of textbooks and monographs may need to be done. And some failure of communication between two generations of investigators may have to be accepted.

Among the weightings and oppositions of these four criteria, two are conspicuous as foci of conflict among enquirers. One consists of continuity and feasibility taken together and opposed to adequacy and interconnection. The other consists of interconnectivity alone as object of exceedingly passionate loyalty and equally passionate attack.

There is also noticeable connection between interconnectivity and certain choices at the first decision point. High ranking of

interconnectivity and preference for atomic reductions or rationals are often seen together; there is negative correlation with choice of formal-material wholes. These relations are to be expected since scope for interconnection is provided by the first two and excluded by the latter. It is interesting, however, that interconnectivity is more frequently linked to rationals than to reductives, since it would appear as if either would serve the purpose.

RELIABILITY-VALIDITY (DECISION POINT 3)

An effectively instrumental principle of enquiry must almost always include a series of terms of decreasing comprehensiveness and increasing specificity of connection with the data-yielding components of the subject-matter. The invention and debate we have so far discussed concern themselves mainly with the higher terms of the series.

We are now concerned with what happens in the choice of the lower terms of the series. The more general terms of a series limit but do not wholly determine the "lesser" terms. For example, a conception of organism and of structure and function may be developed and accepted with no specified scheme within which to discriminate one structure from another—or there may be two or more alternative schemes. Hence, different executors of specific research programs must usually develop or choose different sets of lower terms with which to work, even though they are operating within the same general scheme. Each of these choices will usually be defended as most efficacious of the available alternatives for guiding successful enquiries, the issue turning on different weightings of two criteria of "success": reliability and validity.

Reliability stands for the following cluster: the extent to which the terms of a research program are free of vagueness and ambiguity; the extent to which the referents of the terms are given distinct and unequivocal location and limit; the extent to which the manipulations and measurements indicated by the terms can be undertaken with precision and repeated with uniform consequences. This is one ideal of enquiry.

Validity stands for a less easily specifiable property: the extent to which the terms of a research program approximate to the presumptive richness and complexity of the subject-matter; the extent to which abstraction, in its sense of simplification by removal from context, is eschewed as a species of cheat. This is a second ideal of enquiry. (It includes *adequacy* and *interconnectivity* but in opposition to *reliability*.)

Reliability and validity thus name two obligations which scientists, collectively, recognize as co-ordinate and complemental. One concerns the knowledge produced. The other concerns the subject of that knowledge. On the one hand, the scientist is responsible for making conclusions which are as clear, dependable and precise as the state of his art permits. On the other hand, he is responsible for seeing to it that the subjects and predicates of these conclusions —the central emphases of his enquiry— be as "telling" as possible, that they represent as much of the extent and complexity of the subject as the state of the art permits.

The contrast of the two is seen throughout the fields of enquiry. One biologist restricts the meaning of "sexual act" (in the human male) to an ejaculation and proceeds to count their frequency and to characterize their circumstances by reference only to the age, sex and species of the partner, if any. Another includes fantasies as well as overt movements, searchings as well as satisfactions, symbols as well as actual objects; and characterizes the circumstances of the act in terms of interpersonal components such as possessiveness; guilt, anxiety and shame; dominance-submission; fusion and confusion of identities. One ethnologist examines friendship by reference to the number of hours per week which each member of a group spends with one other, two others, etc. A second proposes a study in terms of the needs which are satisfied by association, the activities used as the occasion for association, the frequency and intensity of jealousy, possessiveness, sacrifice, etc. One

neuropsychologist seeks only those brain defects whose consequence consists of total disappearance of named activities. Another seeks those which result in quantitative changes of qualities of activities which as yet have no name.

While scientists collectively recognize both these obligations and their bearing on one another (unreliable knowledge of something important and well-verified knowledge of the trivial being but two ways to make "knowledge" worthless), scientists individually put grossly different emphases on one or the other. And no other decision point in enquiry gives rise to so sharp and unfortunate a division of enquirers into opposed and uncomprehending camps. To many validists, the reliableist is a man without imagination or one of timorous caution, or one who deliberately pursues unexceptionable and frequent exhibition of "research activity" while others take on the difficult tasks. To many reliableists, the validist is unsound or romantic; unappreciative of rigor and precision as the marks of science; too easily moved by the vague claims of commonsense; unskilled in the use of Occam's razor.

Some of this blind opposition arises from conditions of work and enquiry, especially on the reliableist side. A subject-matter simplified in the interest of reliability is not simplified merely in thought. It is a genuine article, an artifact of the laboratory or field, created by controlling the conditions of enquiry. The facts ignored in conception are suppressed or held constant in fact. Otherwise, the reduction in validity would have no consequent increase in reliability.

Since this relatively invalid subject of enquiry does exist and in those places where the scientist spends most of his time, it is easy for him to become habituated to thinking about his named subject ("learning," "society," "culture") only in the restricted terms of his research. It is almost as easy for him to come to believe in all sincerity that factors omitted in his created version of the subject but claimed by others to exist are products of those others' fancy or gullibility.

Some of the extremism on the validity side also has circumstantial origins. Physicians, teachers, social workers and other engineers are brought, willy-nilly, into contact with unsuppressed and uncontrolled complexities of subject-matter which their investigative sciences may have perforce ignored. The contrast is dramatic and, under the circumstances, very likely to be seen invidiously. In general, it will appear as the failure of scientific theory to "work." This failure and its significance for the engineer leads easily to the conviction that what was bought at this price —the reliability—is relatively worthless. When the engineer is also an enquirer or when the enquirer is in intimate contact with the engineer (as in the case of medicine generally and psychotherapy especially) this invidious comparison carries over from practice to enquiry and colors the agent's view of his research.

Yet, many of the extremists in both camps are hard to account for in this way. They appear to be temperamentally as well as circumstantially dedicated to minimizing one or the other of the two criteria rather than to maximizing the one they praise. On the reliability side, for example, one finds many scientists who go well beyond persistent use and defense of their particular, simplified version of the subject. They spend time, words, and energy actively denigrating more complex versions and actively criticizing co-workers who value validity more. Whether or not a temperamental preference is involved, a high level of anxiety certainly appears to be.

The division of enquirers between these camps is so nearly equal, and each of the ideals is so strongly held, that open attack by one or the other is as rare in the printed literature as it is common in the bar and bedroom post-mortems of scientific meetings. Rather, the division is exhibited mainly by flying the flag of one's choice. Thus, B. F. Skinner (1938):

". . . In English, for example, we say that an organism sees or feels objects, hears sounds,

tastes substances, smells odors and likes or dislikes them; it wants, seeks and finds; it has a purpose, tries and succeeds or fails; it learns and remembers or forgets; it is frightened, angry, happy, or depressed; asleep or awake; and so on. Most of these terms must be avoided in a scientific description of behavior. . . .

". . . Thus the term 'try' must be rejected because it implies the relation of a given sample of behavior to past or future events; but the term 'walk' may be retained because it does not. The term 'see' must be rejected but 'look toward' may be retained. . . ."

Kurt Lewin (1951) answers:

"The psychologist . . . finds himself in the midst of a rich and vast land full of strange happenings: there are men killing themselves; a child playing; a child forming his lips trying to say his first word; a person who having fallen in love and being caught in an unhappy situation is not willing or not able to find a way out; . . . there is the reaching out for higher, and more difficult goals; loyalty to a group; dreaming; planning. . . . Psychology is out to conquer this continent. . . ."

The same near-equal force of the two ideals leads often, in print, to assertion that the other's love, though it be not lovable to me, is yet to be respected. Thus Clark Hull (1932):

"Now for certain rough practical purposes, the custom of naming action sequences by their goals is completely justified by its convenience. It may even usefully be employed in theory construction, provided the theorist is alert to the naturally attendant hazards. These appear the moment the theorist ventures to draw upon his intuition for statements concerning the behavior (movements) executed by the organism between the onset of a need and its termination through organismic action.

"An ideally adequate theory even of so-called purposive behavior ought, therefore, to begin with colorless movement and mere receptor impulses as such, and from these build up step by step both adaptive behavior and maladaptive behavior. . . ."

In general, the printed expression of the schism takes the form of avowing both validity and reliability as goods of science but moving thence to assertion that one of them is best attained, like the Blue Bird, by pursuing the other.

What exception there may be to temperate statement in print arises from the slight but undoubted advantage of reliability over validity as a status source for contemporary American scientists (excepting physicists and psychiatrists). A validist shares slightly but definitely the askant position of the intellectual, the possessor of moral principles, and the unmarried, adult male.

STABLE VS. FLUID ENQUIRIES (DECISION POINT 4)

Once a set of terms for enquiry in a field has been debated, pretested and settled on, there is usually the opportunity (and the need) to exploit the terms as a program for numerous researches aimed at conclusions about this and that and another instance of the phenomena to which they apply: a thousand primitive societies to be asked the agreed-on set of queries; hundreds of classes, subcultures and regional isolates of a highly structured society; the genetics of any number of characteristics of any number of plant and animal species to be discovered; the structural formulae of an entire encyclopedia of organic compounds.

Research of this kind may be pursued with either of two distinct aims in view. On the one hand, attention may be focused exclusively on the subject-matter itself—the cultures, genetic characters, organs, functions, and so on. The terms, as terms, are ignored, especially those which stand for the subject-of-enquiry itself. The fact that the terms are tentative—perhaps incomplete, perhaps inappropriate—and to be tested by determining how and where they fail in the course of detailed enquiries is not permitted to divert the investigator from his interest in the subject-of-enquiry taken as subject-in-fact and from his hope of positive conclusions about it. This is to be called stable enquiry.

On the other hand, after the invention and pretest of terms through debate, there must be extended test of the principle by practice. The extended test involves the pursuit of data and the formulation of conclusions as do researches of the stable sort, but experiments are performed, data educed and interpretations tried in order to test the terms in which the enquiry is pursued. The subject-of-enquiry is taken as a probably imperfect image or model of some

supposed subject-in-fact and research in the terms dictated by the subject-of-enquiry is pursued with the aim of discovering and repairing its inadequacies and limitations: whether the subject will speak to the problem as posed; whether the data required can be elicited with the necessary accuracy; whether data will be found for which the terms of the enquiry cannot give an account. This, I call fluid enquiry.

Enquiries pursued as search for "conclusions" may, in many cases, be utilized by others for this second function, but, on the whole, significantly different behavior is required of the scientist in each of the two cases. Stable enquiry lends itself to segmental structuring. Discrete research programs of relatively short duration can be formulated. One may select for investigation those particular members of the set defined by the principle (e.g., organs, traits) which will cause the least trouble; in genetics, for example, hair color, a sixth digit, web feet, which are easily identified either-ors, as against corolla length, or intelligence (which exhibit continuous variability) or schizophrenia which is equivocal and hard to identify. What is required by way of data is unequivocal. The pattern of experiment is laid down. Each program can be envisaged in terms of a definitive termination. Precision, thoroughness, and "technique" are maximized as the criteria of excellence. In the test of principles, on the other hand, the duration of the enquiry is indefinite; what will constitute the significant data is unknown; the outlines of a satisfactory outcome are vague or invisible. In general, the task is not to traverse a marked route but to determine a route and a destination and to do so with uncertainty as a conspicuous factor at each step.

Throughout the biological sciences, in many of the behavioral sciences, in chemistry and geology, there are regiments of journeyman scientists who carry out only stable enquiries while fluid enquiry is carried out by squads or retained as the special privilege and power source of a "Staff." Stable researchers are usually unaware of the existence of fluid enquiry. Where they are aware of it, they are uneasy and offensive. The common epithets are: *just words, speculation, thinking.*

There is reason to think that here, as at the other decision points, psychological factors are operative. Some factor of tolerance of ambiguity appears to characterize the fluid enquirer (as it does the seeker of validity) together with pleasure in the creation of new structures and, perhaps, some sense of exceeding or disposing of fathers. Stable enquirers are often men of fixed habit in other respects and given to worry about nutrition, constipation, and their children's future.

However, there is also operative here, as at no other decision point, a social machinery for encapsulating young scientists into the stable mode and keeping them there. Entire graduate curriculums exist which afford no hint that principles are matters of invention and alteration but, rather, treat current knowledge and practice of a field as its *echt* definition. The practitioner extruded from such a program is later kept in line by his ignorance, by his developed incapacity to understand reports of fluid enquiry and by the system of refreshment programs and special texts by which he is given only as much of new material as his continued usefulness may require.

How far, then, the stable enquirer is a product of his education and how far he selects himself for a limited career is to be determined.

GUIDING, COLLECTING, INTERPRETING (DECISION POINT 5)

I doubt whether this decision point is of much importance and shall treat it accordingly.

Of the numerous possible divisions of the labor of enquiry, three are commonly discriminated as choices by some men of science. There are those who concern themselves primarily with the invention and proposal of principles. There are those who delight in the meticulous and careful accumulation of data for interpretation by others. There are still others who engage primarily in the work of review and monography.

The first of these choices is marked by

its variable status among the different sciences. Physics honors the "theoretician"; chemistry, its chemical physicists; psychiatry, its Freud and Sullivan; sociology, its Parsons and Durkheims. Geology honors dead ones. Biology tends to reject the role out of hand. Indeed, I recall only two documents in the biological literature which treated a theoretical problem in theoretical terms and found acceptance: the brief letter by Hardy, the actuary, which initiated the development of modern genetical evolution; and a few publications by Sewall Wright in the same field. Usually, biology will tolerate a theoretic contribution only when delivered in a context of experiments done and data collected. Even a field which tolerates theoretical work often demands a price of admission to the activity: an apprenticeship in the sweaty phases of enquiry. Even Einstein served a novitiate. These social restrictions mean that an attempt to study the character and competence of the theoretical man must look closely to its samples. A sample of non-theoreticians is likely to be adulterated by some number of unidentified theoretics.

Of the remaining two choices, a word on the third is in order. The activities of review and monography in the biological sciences (including the clinical) are signs and sources of professional political power. The task of the monographer is to relate diverse researches. The conclusions of such researches are usually less than certain and often contradictory. Their apparatus of terms and techniques will often exhibit wide variability, in respect of generals as well as specifics. The integration of such a diversity requires judgment of the reliability and validity of numerous researches, and in the process of exercising that judgment a monographer has the chance to influence the future course of a field of enquiry and to affect individual careers for better or worse. His power is restricted, in some degree, since his subjects and his audience are largely the same persons, but this restriction is neither greater nor less than that of any elected officer of a polity. Indeed, among monographers, there is a noticeable number of persons who exhibit the social habit of the elected administrator: crony-gregariousness with its collection of loosely held, largely interchangeable, partners in dining, drinking and story-telling; disinterest in a life of continuity and consequent opportunism.

THE *O*-POINT OF DECISION

We began our analysis of enquiry by supposing that all scientists embark upon enquiry with the intention of contributing to our mastered knowledge of the subject-matter. This supposition must now be denied. There are two other starting points.

Let it be supposed that in most fields at most times, consensus can be obtained as to the important and outstanding problems yet to be solved. Let the ideal of our original supposition consist of efforts at solving one or more such problems. The enquirer eyes the subject-matter as known and as it might be known, and the disparity he sees there constitutes his starting point.

The second choice of starting point is familiar enough: not the subject-matter and the holes in the science of it, but techniques and instruments, intellectual or otherwise: path-coefficients, factor analysis, game theory, electron microscopes, computers, tape recorders. The enquirer has his favored instrument and is master of it. Like a child with a hatchet, he looks for something to chop.

The third choice is neither hole in science nor instrument mastered, but virtuosity, *per se*. The enquirer believes he excels others in ingenuity, inventiveness or what not and looks for means to exhibit it. His starting point is any thing or situation through which he can exhibit his skill. His relation to like-sexed peers is reminiscent of Don Giovanni's and worth investigating.

Persons of the second and third persuasion are probably useful to science in about the degree to which they are tolerated at any given time. The tool master introduces new tools on the one hand, and on the other, tills stony acres. The ingenious, by seeking novelty, occasionally find it.

POSTSCRIPT ON A GRAND STRATEGY

Insofar as this document implies a view of enquiry, a word is in order asserting what is otherwise only suggested—that there

are many ways of achieving mastery of a subject of enquiry, no one of them capable of undebatable superiority over the others; each of them capable of illuminating the world of things in a way not precisely duplicated by the others.

Many of these complementarities are obvious. Rational principles confer coherence on what otherwise would be dissevered by holistic principles. Reduction confers coherence in another way. Meanwhile, holistic atacks on problems emphasize the peculiarities of local subject matter while rational and reductive theories tend to subordinate what is local and special to what is common among many subject-matters.

Even bull-headed reduction has advantages over its more sophisticated brother in the course of the actual practice of enquiry. For the enquiry which begins by looking to the constituted to tell it the behaviors and qualities to be accounted for is likely to look to the constituents to account for these matters only. The enquiry which looks first to the constituents, on the other hand, is a freer enquiry precisely to the extent that it is unguided. A similar advantage accrues to anti-principled enquiries.

We need not, therefore, *make* a virtue of the necessity of pursuing enquiry through men who are moved by numerous preferences to work in different ways toward differing specifications of their common goal. It *is* a virtue.

Yet, as long as resources for research are limited, there is an itch to believe that one of the several strategies available to science is the best one. It would be very nice to be able to plan the course of a new enquiry and at different stages in the process select enquirers for their appropriate predilections as well as for appropriate abilities.

I have tried to show that this hope must betray us. Consensus on a single pattern of choices will merely enable us to overlook what we have not done in our enquiry. Different principles maximize different dimensions of successful enquiry. One hierarchy of criteria merely legislates definition of the goal. A single ratio of fluid to stable enquiries imposes an arbitrary relation between theory and practice, future and present goods.

This leaves the possibility that some particular *order* of different strategies, constituting a grand strategy may be better than all other orders. This hope may be realizable, though there are two vitiating considerations. In the first place, the possibility of a grand strategy assumes that different subject-matters are alike as subjects of enquiry. Yet a good historical case can be made against this premise as well as for it, and a good metaphysical case has, of course, been made on either side. A good scientific case would entail a trial of all such grand strategies on all subjects of enquiry, which amounts to a confusion of types.

In the second place, if we grant that all subjects are alike as subjects of enquiry, we encounter a second and similar trouble. The grand strategy best suited to this homogeneous subject of enquiry may not be the same as the one best suited to the human species *qua* enquirer—a modern instance of the ancient distinction between what is most knowable without qualification and what is most knowable to *us*. And even if these coincided, we must determine whether that common grand-strategy coincides with the one best suited to a generation of enquirers caught in a particular web of circumstance—the state of enquiry in other fields; the state of development of instruments of enquiry, such as logic, mathematics, and high-voltage accelerators—at the time that enquiry on some given subject is initiated. In short, the problem of a best grand-strategy is a prudential one, contingent on the time, the place and the subject of enquiry. Within these caveats, I shall now record a speculation on a grand strategy—specifically a grand strategy for subjects far from physics on the Comteian scale, for attack by American scientists, of enquiries instigated in the milieu of current fashions and conditions of enquiry.

Speculation on a grand strategy

In the first place, I am convinced that a rational principle should be among the

last to be tried. Its early use must be either an illusory and damaging "success" or a wasteful delay of prerequisite enquiries.

These unfortunate possibilities rest on the point that rational enquiries involve the matching of two well-developed bodies of material. On the one hand, there must be the rational scheme itself, whether a geometry, a probabilistic system, some other calculus or a configurative corpus. On the other hand, there must be a body of *relatively* unorganized propositions representing the subject of the enquiry. The rational investigation is completed when one such component is well-matched with the other, when stable relations are established somehow or other, as Einstein put it, between concepts and sense experience.

Now it is pretty obvious that the way to use a rational principle prematurely is to try for the third step when the first two have not been taken an optimum distance. My concern here is with what constitutes the optimum for the second step—development of propositions representing the subject of the enquiry. I shall pretend, knowing little about the matter, that a plethora of rational schemes exist. If I am wrong about that and they too are in need of expansion for the purposes of biology and the behavioral sciences, then I shall be right in my conclusions, a fortiori.

Readiness of a body of propositions for application of a rational scheme involves two criteria: First, the body of propositions must bear a reasonable resemblance (in respect of validity and reliability) to the subject matter. Second, they must exhibit some sort of middle ground between complete variety and complete coherence.

The sense of the second of these conditions calls for some elucidation. The point is this: a highly heterogeneous body of propositions means propositions derived and couched in dozens of different and independently developed sets of terms (as might arise from dozens of workers held together by no achieved consensus about the problems of the field or the character of its subject-matter). The independence or "irrelevance" of these terms is what a rational principle is designed to overcome. The number of them is the factor which bears on the question of timing the search for a principle.

Now let us see how these two conditions—the validity of the propositions and their degree of heterogeneity—bear on the success or failure of a rational enquiry. Suppose we have only a few propositions (or many, but couched in a few sets of terms) representing the subject-matter. Then the probability is very high that they are low in validity—lacking either adequacy or scope as far as the subject-matter is concerned. They will represent either a highly simplified slice through the whole subject-matter or a thorough analysis of some small fragment of it. Now suppose that someone successfully fits a rational scheme to these propositions. Who would deny that his success would be a constraining bind on further enquiries? He has a structure whose separate propositions are "true" (reliable). And the structure is a coherent whole. That combination is highly seductive. The vast majority of workers in the field will fall into line and subsequent enquiry will continue in the terms of the "successful" rational principle for a long time—erecting a large, attractive and illusorily adequate body of knowledge of the subject. For nothing marks off the American scientist more than his readiness to ignore when he can the distinction between the subject as emboded in his terms and the subject as it may be.

If, on the other hand, such a consensus be postponed for awhile—by the absence of a "successful" rational scheme—we leave time for more numerous, relatively independent attacks on the subject matter. Attacks on different facets of it enlarge the scope of the growing body of propositions. Attacks in different terms test out the validity of earlier terms and amend them where they are wanting.

Now if *this* goes on too long, we exceed the second condition for effective rational attack. We get a large collection of heterogeneous propositions, hence a very large number of small sets of terms and our problem shifts from one of prematurely binding coherence to one of great difficulty in achieving coherence at all. We have too

many clues to the kind of rational scheme which would work or, putting it another way, too many conditions which a rational scheme must meet at one fell swoop.

At this point, I appear to contradict myself, for I have claimed that rational attacks should be made late in enquiry while my argument so far suggests a midpoint rather than a late one. The remaining step in the argument consists in showing that we can have our cake and eat it too—extend enquiry and thus increase validity without running into an excessively large number of sets of terms. We can do it by introducing *local* and *interim* coherencies by means of attack with reductive and holistic principles.

Thus, the initial phase of our grand strategy would consist of use of primitive and anti-principles. The second phase would consist of the binding of the results of these enquiries into a small yet varied number of holistic and reductive views, each of which would go its own way for awhile, leading to more comprehensive reductions, then to some very few less-than-comprehensive rationals and only then to a sweeping rational "synthesis."

Objections to argument

There are a number of apparent holes in this speculation about a grand strategy. Here are two of them.

It may be argued that the strategy is planned entirely in the interest of validity (both interconnective and adequate). Not at all. Reliability is not sacrificed. Continuity and feasibility are also maximized.

Reliability is maintained in the sense, first, that the strategy is a strategy and not a tactic. In the gathering of the propositions which shall constitute the matter for ultimate integration we assume due regard for precision and consistency of measurement and observation. Second, by asking that early principles stay close to the subject matter, we let the subject matter and the enquirer's competence rather than the terms of a rational principle lead the enquiry. The individual enquirer in close and relatively undistracted connection with the subject-matter determines what will constitute his data. Hence, as long as he affirms reliability as a scientific good, reliability will not be sacrificed for validity. This same argument speaks for feasibility.

This brings us to a second hole—the fact that we propose that reductive and holistic principles, rather than small-scale rationals, do the job of local and intermediate integration. There are two reasons for this, the first of which involves a qualification of that proposal. That first reason is that rationals enjoy entirely too much status at present. (Remember, we said that this strategy was contingent and prudential.) In their own right there is nothing against rationals serving *pro tem* integrations. But the passion of modern scientists for rationals is so abject that, in their eyes, the first rational in the field is likely to assimilate what propositions it can and relegate the remainder to the category of irrelevance rather than leave room for additional local and *interim* integrations.

The second reason for rejecting rationals as intermediate integrators stems from the fact that we are discussing modern enquiries in subject matters high on the Comteian line. This combination is equivalent to remarking that knowledge of subject matters lower on the line are, in fact, reasonably well-developed. If they are—and they are—it would be inefficient and a waste to ignore them. In short, there is much good material available for reductive attacks and that material should be used. This can be done with reasonable safety since the seductiveness of reduction is so much lower than that of rationals just now and because reductives easily invoke holistics as competition. (E.g., Durkheim after Mill; Skinner after Pavlov, et al.)

As to continuity, reductives and holistics supervening on primitives and anti-principles have an inbuilt continuity in actual enquiry. Primitives, as practicals, must speak in terms of prediction or control. Since we are discussing a subject-matter not yet known in rational terms, controls and predictions of aspects of it must stem in large part from knowledge of the behavior of its constituents (since this is the only large body of knowledge available). Residual items of prediction and control must come from *ad hoc* items of information

about the subject-matter taken in terms of its own properties. This amounts to saying that primitives will be fragmented bits of reductives and holistics, hence that a movement to systematic use of reductive or holistic principles will stem from the primitives and incorporate them. Similarly, "Laws of Nature" and "Causes" will also speak in the terms of known constituents or recognizable properties of the subject, hence will similarly contribute to and be assimilable to the reductive and holistic principles which may supervene.

Yet, my imaginary critic may have a point here. It may be that interim rationals are more easily assimilable to a large-scale rational synthesis than holistics and reductives would be. If that be the case, we have a choice between validity and continuity. I would choose validity.

APPENDICES

Appendix 1. Some problems in a study of enquiry.

The framework of decision points and choices has been relatively empty of information as to what kinds of persons make what decisions for what utilities. Such information has been suggested as one main line of enquiry for which the framework would be useful.

Eight other lines of enquiry are as follows:

1. To what extent are choices responsive to the exigencies of a subject matter rather than expressive of personal preference? E.g., is there a great difference between the conceptions appropriate to investigation of the organism and those appropriate to investigation of nonliving subjects?

2. To what extent are choices responsive to the state of enquiry at a given time rather than expressive of personal preference? For example is the biologists' passionate avoidance of final cause and "anthropomorphism" in the period 1915–1935 a reaction to errors which arose from uncritical use of these notions in preceding decades?

3. To what extent are choices indicative of fashions generated by treatises on method or well-publicized new instruments and models for research? E.g., the radical empiricism of W. K. Clifford and Karl Pearson; "operationalism" more recently; "probabilistic" structures; Game Theory?

4. To what extent do choices reflect a "natural evolution" of enquiry in each field? E.g., from description to causation to quantification, or Euclidian geometry to algebra to partial differential equations? (Without prejudice to the possibility of respectable enquiry along this line it should be remarked, nevertheless, that probably no ordering of no set of "methods" or "principles" but has been asserted out of hand to be the evolutionary sequence of science—usually as an argument for a favored principle. Thus: taxonomy, static laws, dynamic laws; or analytic, summative, synholistic; or theological, metaphysical, positive.)

5. To what extent does amount of variance in respect to choices in a given field at a given time reflect the state of communication? E.g., when scientists in a given field read the same noncompetitive journals, belong to the same societies and attend the same meetings, to what extent does variety of strategy disappear? If variety does disappear, does consensus arise via suppression of preferences or via self-selection of those entering the field?

6. To what extent do the choices at the several decision points of given enquirers constitute whole strategies or patterns of choice? I.e., to what extent is choice of *a* at decision point *1* correlated with choice *b* at decision point *2*? I treat each decision point independently of others but the apparent connection between some choices (as choices) is too obtrusive to ignore entirely.

7. What variability exists among scientists with respect to the existence of preference or to efforts at conscious control of personal preference in the interest of such other desiderata as 1 and 2 above? What personality traits mark controllers and indifferentists? This line of enquiry is especially desirable as a countercheck to the psychological relativism of the main enquiry. This countercheck is especially important in view of the fact that though hundreds of scientists exhibit marked preferences in respect to the choices to be described, a few exhibit none and habitually combine what others treat as mutually exclusive.

8. Certain aspects of the personal history of the enquirer are interesting as possible correlates with choices made: (a) stage of career, age, etc. (b) the preferences of professional father figures, as thesis chairmen, beloved teachers, first boss.

Appendix 2. On "emergent" principles

As far as I can determine, no enquiries are conducted in terms of emergency and no emergent phenomena present themselves for enquiry except by way of an artifact arising from hypostasization of atomic reductive principles. The process appears to run as follows.

When durable reductive principles prevail for a considerable time in a field of investigation, the success of the principle gives rise to an orthodoxy which asserts not merely that reductive principles alone are scientific but that "nature" is reductive. Enquiry then encounters a phenomenon which resists explanation in reductive terms but is successfully investigated holistically. Now, since the phenomenon and the account of it are not discriminated, the combination of the two, misidentified as the phenomenon pure and simple, is seen against the background of phenomena reductively explained as something strange and novel—"extranatural." The novelty in turn

becomes something to be explained and this is done by distinguishing two orders of phenomena: ordinary ones and those which arise without due and proper basis in characteristics of the constituent factors. The latter phenomena are dubbed emergent. Of course, their emergency disappears if new properties of the constituent factors are sought and found; or if the earlier body of knowledge in the field is translated from reductive to other terms; or if the phenomena and the mode in which they are investigated are discriminated.

Appendix 3. On Occam's Razor

There is a division of scientists with respect to their valuation of economy as a criterion of principles. Among users of rational principles and hunters of historical causations there is much obsessive concern for the minimal while the bulk of scientists, though willing to delete an unnecessary term when it is pointed out by others, are more than willing to let others do the pointing. They seem to be able to take economy or leave it alone.

Whether this indifference is, in fact, true of the bulk of scientists cannot presently be determined. Attitudes about economy tend to be indistinguishable because the degree of economy itself (gross redundancy aside) involved in a structure of terms is hardly capable of measurement. At least, I am not aware of a well-accepted definition and measure of the quality.

For this reason, I omitted economy from the set of choices at this point.

Appendix 4. On relativism

The charge of relativism can fairly be laid against the viewpoint of this paper but there is a difference between the relativism of Judas (What is truth?) and that of John Smith (There is more than one way to skin a cat!). Two points are germane here.

First, if scientific knowledge can be sought in many ways, it is not because science is a game, a systematic delusion or the pursuit of metaphors and mnemonics. Rather, to paraphrase Professor Warner Wick, it is because nature is so rich in matters to be learned and scientists so apt at finding ways to learn them.

Second, there *are* consequences to a choice of principles and it is *not* all one which principle is chosen. Principles not only initiate enquiry; they are tested by it. And if what is tested is not the propriety of the principle to the subject-matter alone but to the manifold of matter, enquirer and circumstances of enquiry, that too redounds to the benefit of the enterprise.

REFERENCES

Aristotle. *Parts of animals*. Book I, Chapter I.

Boas, Franz. *Race, language, and culture*. New York: Macmillan, 1955.

Clifford, W. K. *The common sense of the exact sciences*. New York: Knopf, 1946.

Comte, A. Course of positive philosophy, Chap. II, Tr. A. C. Benjamin, in *History of the organization of the sciences*. Chicago: Univ. of Chicago Press, 1943.

Durkheim, Emile. *The rules of sociological method*. Glencoe, Ill.: The Free Press, 1950.

Eccles, J. C. *The neuro-physiological basis of mind*. Oxford: Clarendon Press, 1953.

Einstein, Albert, *Physik und realitat*. J. of the Franklin Institute, March, 1936. Translated in *Methods of the sciences*, 6th ed., Chicago: Univ. of Chicago Press, 1955.

Haldane, J. S. The universe in its biological aspect. In *Materialism*, London: Hodder & Stoughton, 1932.

Hull, Clark L. The goal-gradient hypothesis and maze learning. *Psychol. Rev.*, 1932, 39, 25–43.

Hull, Clark L. *Principles of behavior*. New York: Appleton-Century-Crofts, 1953. Chs. I and XX.

Koffka, Kurt. Perception: an introduction to the Gestalt-Theorie. *Psychol. Bull.*, 1922, 19, 531–581.

Lewin, Kurt. *Field theory in social science*. New York: Harper, 1951.

Lewin, Kurt. Group living: autocratic, democratic, and laissez-faire. In W. Ebenstein (Ed.), *Modern political thought*. New York: Rinehart, 1954, pp. 89–97.

Mach, E. *The science of mechanics*. LaSalle, Ill.: Open Court, 1942.

Maslow, A. H. *Motivation and personality*. New York: Harper, 1954.

Needham, J. & Baldwin, E. (Eds.). *Hopkins and biochemistry*, 1861–1947. Cambridge: W. Heffer, 1949.

Skinner, B. F. *The behavior of organisms*. New York: Appleton-Century-Crofts, 1938.

von Bertanlanffy, L. A program of systems research and interdisciplinary synthesis. Center for Advanced Study in the Behavioral Sciences, Mimeograph. Undated.

von Helmholtz, H. Uber die Erhaltung der Kraft, Berlin: J. Reimer, 1847; Tr. H. Stein, in *Relation of physical and biol. sciences*. Chicago: Univ. of Chicago Press, 1955.

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Different results from studies in problem solving conducted 25 years apart have led these authors to a consideration of the part increased experience with puzzles and problems over a generation has played in producing such differences. The relation of previous experience to the level of aspiration of the individual, alone and operating in groups of varying sizes, is detailed in this study.

GROUP AND INDIVIDUAL PERFORMANCE IN PROBLEM SOLVING RELATED TO PREVIOUS EXPOSURE TO PROBLEM, LEVEL OF ASPIRATION, AND GROUP SIZE

by Irving Lorge and Herbert Solomon[1]

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INTRODUCTION

In duplicating Shaw's (1932) experiments in group and individual problem solving, Lorge and Solomon (1959) found that their 1957 result for the proportion of successful solutions by individuals was markedly higher than those reported by Shaw in 1932. In fact, the 1957 proportions were very nearly like those reported by Marquart in 1955. The difference between the 1957 and 1932 data may be charged to the difference in the characteristics of the subjects, or as seems more likely, to differences in the extent of prior exposure to the problems that were offered for solution. Variations in experimental results over a twenty-five year period may be attributable to increased experiences with puzzles and problems over a generation.

The discrepancy in results between the generations, therefore, may affect the relevance of the mathematical model proposed by Lorge and Solomon (1955) to account for the superiority of groups over individuals in Shaw's data. Therefore, the experiment reported here was designed to test the hypothesis that previous exposure with the problem would significantly affect the proportion of individual solutions and, hence, the proportion of group solutions. Since the model states that the proportion of group solutions is related functionally to the size of the group, the experiment was designed to get estimates about the proportion of successful solutions by individuals and by groups of sizes three, four, six, and seven. Since Lorge and Solomon had used individuals as well as groups of size two and five in their 1957 experiment, the sizes of the groups scheduled would complete the range of group size from two through seven employed in the solution of the Tartaglia problem (Kasner & Newman, 1940), a transportation puzzle given in detail in the next section.

The central problem was to assess the model for expectation of group performance, proposed by Lorge and Solomon, namely, that if

P_G = the probability that a group of size k solves the problem, and

P_I = the probability that an individual solves the problem, then

$$P_G = 1 - (1 - P_I)^k . \quad (1)$$

This essentially states that if at least one member of the group has the ability to solve, the problem will be solved. Further, if the problem must be solved in stages, then a second model can be obtained by having model (1) apply at each stage. The authors do not suggest that this rationale is the basis of problem solving by groups, but rather that it was successful in "explaining" Shaw's observed data. Essentially, the model does serve as a measure of the extent to which *ability* of individuals in the constituted group is the sole factor in solutions by groups and,

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if the model does not hold, whether it is necessary to assume that some positive or negative personal interaction among the individual members of the group is affecting the results.

When the observed proportion of group successes, p_G, and the observed proportion of individual successes, p_I, approximately satisfy equation (1) then ability may be considered to be operating as the only factor. When p_G is much higher than $\hat{p}_G$ where $\hat{p}_G$ is the solution for P_G in equation (1) for observed p_I, factors other than ability alone may be operative; likewise when $\hat{p}_G$ is much smaller than p_G some factor or factors may be restricting ability or may be preventing ability to show itself. It should also be mentioned that when p_G is much higher than $\hat{p}_G$, it is still possible for ability to be the sole factor but the problem can be solved in stages requiring ability interaction at each stage (Lorge & Solomon, 1955).

THE EXPERIMENT

The primary object of the experiment was to estimate the effect of previous exposure to a problem not only on individual, but also on group performance, and thus evaluate how previous experience may influence the relation given by equation (1). Groups of size three and four were employed to duplicate previous experimentation and get some check on variability of group performance; and groups of size six and seven were employed to extend the range of group sizes.

The subjects were graduate students, primarily candidates for the M.A., at Teachers College, Columbia University in attendance at the 1957 Summer Session. Two different classes participated: one was a class in "Education and Society," the other a class on "The Work of the Teacher." Students in both classes may be assumed to be equivalent in mental ability. Thus, in this experiment, measured intelligence was not used as a control variable; moreover the predominant concern was with amount of previous exposure.

The first class, identified as Class 1 in the Tables, comprised 290 students, of whom 89 participated as individuals, the others as 23 groups of three and 22 groups of six. The second class, identified as Class 2 in the Tables, comprised 202 students, of whom 37 participated as individuals and the others as 15 groups of four and 15 groups of seven. All assignments to serve as individuals or as groups were made by a random procedure.

The problem for solution was the Tartaglia transportation puzzle. In order to get some estimate of the extent of previous exposure to this problem, each student was given a mimeographed sheet which gave the problem, and asked six questions regarding the student's retrospective memory about previous experience with the problem or with problems like it. In addition, each student was requested to estimate his ability to solve the problem within a 45-minute period. The question about "level of aspiration" was intended to discover whether some students would tend to regard the problem beyond their ability to solve. The sheet, in addition to requesting the student's name and assignment number, gave the problem and the questions as follows:

On the A-side of the river are wives (W1, W2, W3) and their husbands (H1, H2, H3). All of the men but none of the women can row. Get them across to the B-side of the river by means of a boat carrying only three at one time. No man will allow his wife to be in the presence of another man unless he is also there.

1. Have you ever seen or heard this problem before?
   Yes ___ No ___
2. Have you ever seen or heard one like it before?
   Yes ___ No ___
3. Have you ever attempted to solve this problem before?
   Yes ___ No ___
4. Have you ever attempted to solve one like it before?
   Yes ___ No ___
5. Have you ever solved this problem before?
   Yes ___ No ___
6. Have you ever solved one like it before?
   Yes ___ No ___
7. In your opinion, do you think you can successfully solve this problem in 45 minutes?
   Yes ___ No ___

Since subjects were assigned as individuals or to groups randomly, it is obvious that there was no control over the assignment of subjects on the basis of varying degrees of prior information. Such a control, desirable as it may have been, was beyond practical consideration. Because of this limitation, as-

TABLE 1

STUDENT'S REPORT ON PREVIOUS EXPERIENCE WITH SIMILAR OR THE SAME PROBLEM: BY ASSIGNMENT AS INDIVIDUALS AND IN GROUPS

Assignment as	Number	No experience with similar and same problem	Some experience with similar but none with same problem	Some experience with similar and same problem	Some experience with same but not with similar
Individuals	126	42	72	8	4
Groups of 3	69	26	39	4	0
Groups of 4	60	16	34	7	3
Groups of 6	132	39	72	21	0
Groups of 7	105	40	43	18	4
Total	492	163	260	58	11

signments to groups of four, six, and seven did not yield as neat a distribution of the prior experience as would have been desirable.

The amount of prior information that each subject was presumed to have was based on the responses to the questionnaire. Responses to items 1, 3, and 5 give the reported extent of previous experience with the Tartaglia problem, and responses to items 2, 4, 6 give the reported extent of previous experience with problems similar to the Tartaglia.

The items about the Tartaglia or about similar problems may be thought of as scaled in the sense that solution presupposes attempts to solve, and attempts to solve presuppose having heard or seen the problem previously. Thus, prior experience with a similar problem could take the values of 3, 2, 1, or 0, and prior experience with the Tartaglia problem could take on the same values. In recording the extent of previous experience, the value for experience with similar problems was specified first and then experience with the same problem, second. A third notation was made to show the response to question 7 on "level of aspiration"; e.g., 2-1Y would indicate that the student had answered affirmatively the first two questions regarding his experience with a similar problem, the first question regarding his experience with the same problem, and "Yes" to his opinion that he could successfully solve it in 45 minutes.

The data, therefore, were classified by the "level of aspiration" and the extent of experience with a similar and/or the same problem. About a third of all students in both classes reported that they had no previous experience with a similar problem and with the same problem [Table 1] and more than half (53 per cent) did not report any experience with the Tartaglia problem. Almost three quarters of all the subjects reported that they thought they could solve the problem in 45 minutes [Table 2].

TABLE 2

STUDENT'S REPORT ON BELIEF THAT THEY COULD COMPLETE TARTAGLIA PROBLEM IN 45 MINUTES: BY ASSIGNMENTS AS INDIVIDUALS AND AS GROUPS

Assigned as	Number	Number reporting		
		yes	no	omit
Individuals	126	89	35	2
Groups of 3	69	42	26	1
Groups of 4	60	45	14	1
Groups of 6	132	89	43	0
Groups of 7	105	92	13	0
Total	492	357	131	4

ANALYSIS OF EXPERIMENTAL RESULTS

Table 3 gives the gross results of the two classes. The first class certainly does not demonstrate much superiority for groups, while the second class yields results more in line with previous data. In fact, the results for groups of four and for individuals in Class 2, namely $p_I = .16$, $p_G = .66$, are re-

TABLE 3

INDIVIDUAL AND GROUP PERFORMANCE IN SOLUTION OF TARTAGLIA PUZZLE: AUGUST, 1957

	Number participating	Number of solutions	Number of no solutions	Proportion of solutions
		Class 1		
Individuals	91	13	76	.15
Groups of 3	23	3	20	.13
Groups of 6	22	4	18	.18
		Class 2		
Individuals	37	6	31	.16
Groups of 4	15	10	5	.66
Groups of 7	15	7	8	.46

markably close to Shaw's results of twenty-five years ago, namely $p_I = .14$, $p_G = .60$. The results of p_I, for Shaw and the two classes, demonstrate remarkable stability, namely .14, .15, and .16; on the other hand, results reported by Marquart (1955) and our April, 1957 (1959) experiment also show some stability but around larger values of p_I, namely .33 and .41, respectively. It was, of course, these discrepancies which suggested prior exposure as a relevant variable. In the current experiment, however, the values are not different from those of Shaw. Of course, many factors may affect the discrepancies in the values observed for p_I. For instance, the students in the two classes being reported were much like the Teachers College students used by Shaw, while those in a psychology course at Teachers College in the April 1957 experiment were like Marquart's psychology students at the University of Arizona. Obviously it would be good to have some idea of the variability of p_G for groups of different sizes so that some intelligent decisions can be made about manifestly large differences in p_G in repeated experiments for groups of the same size. In any event, that the variance of p_G is a function of both group size and P_I is obvious and is developed in the section on Variance of $\hat{p}_G$ under the assumption that the model given by (1) is operating. Employing some of the results given in Table 10 we notice that the range of maximum variability occurs in the vicinity of our experimental parameters. For example, our use of groups of size three and size four are in the area of maximum variability of $\hat{p}_G$ for the Tartaglia puzzle where a reasonable value for P_I is .15. If N_I is 100, then $\sigma^2_{\hat{p}_G} \sim .0060$ for $k = 3$, and $\sigma^2_{\hat{p}_G} \sim .0077$ for $k = 4$. This gives $\sigma_{\hat{p}_G} \sim .077$; $\sigma_{\hat{p}_G} \sim .088$. For groups of size three and $p_I = .15$, $\hat{p}_G = .39$; for groups of size four, $\hat{p}_G = .48$. Since the corresponding observed p_G's in this experiment were, respectively, .13 and .66, the former for $k = 3$ and much smaller than expected, and the latter for $k = 4$ and much larger than expected, the difference, if the model is operating and $P_I = .15$, is only three standard deviations for $k = 3$, and two standard deviations for $k = 4$ and thus not entirely outside chance considerations. This discussion is not meant to provide a support for the model especially under the somewhat anomalous data obtained but to demonstrate how the magnitude of the variability can permit more reasonable decisions.

It is significant that no individual who had previously indicated that he did not believe that he could solve the Tartaglia problem did actually solve the problem working as an individual regardless of previous experience with the Tartaglia problem or problems like it. Of the 89 individuals working as individuals who believed that they could solve the problem, only 18 solved it, i.e., 20 per cent. In all, 23 individuals assigned to solve the problem individually reported no experience with a similar and the same problem but believed they could solve it. Of these 23, seven solved, i.e., 30 per cent. Of the 54 individuals in both classes who reported some experience with a similar problem but not with the same problem and who believed that they could solve the problem, nine solved, i.e., 17 per cent. In both classes, there were twelve individuals assigned to work as individuals who had some previous experience with a similar and the same problem and who thought they could solve the problem. Only two solved, i.e., 17 per cent.

Since none of the individuals who did not expect to solve the problem did so, it seems reasonable to infer that "aspiration" is a significant factor in problem solving. Of course, a No response to Item 7 could reflect a genuine self-report of lack of ability. Therefore an estimate for P_I could be based on those individuals who had no experience with the problem and who also think they can solve the problem, i.e., 18 out of 89 or 20 per cent. But any ability model as such must give consideration to the fact that those who believe that they cannot solve the problem are among those usually affecting the proportion P_I. Therefore a more realistic estimate of P_I is given by regarding all individuals working as individuals but who had no experience with the problem. This gave 19 solutions for 126 attempts, i.e., 15 per cent. This value should be especially contrasted with Shaw's value of 14 per cent (3 out of 21) which occurred in 1932. Nevertheless, four individuals who believed that they could solve the problem reported that they had solved the problem previously. Of these four, two solved

TABLE 4

SOLUTIONS TO TARTAGLIA PROBLEM BY INDIVIDUALS WORKING AS INDIVIDUALS, CLASSIFIED BY ASPIRATION TO SOLVE AND BY PATTERN OF PREVIOUS EXPERIENCE WITH A SIMILAR OR THE SAME PROBLEM

Number of Yes's to Question 7	Pattern		Class 2		Class 1	
	Similar	Same	Solution	Fail	Solution	Fail
Yes	0	0	4	7	3	9
	1	0	0	3	2	3
	2	0	0	3	2	7
	3	0	2	8	3	21
Yes without previous experience on same problem.....			6	21	10	40
			—	—	—	—
	0	1	0	1		
	1	1	0	1		
	2	1	0	1	0	1
	0	2	0	1	0	1
	2	2	—	—	0	1
	3	2	—	—	0	1
	0	3	—	—	1	0
	2	3	—	—	0	1
	3	3	0	1	1	0
Yes with previous experience on same problem..			0	5	2	5
			—	—	—	—
No	0	0	0	1	0	16
	1	0	0	1	0	3
	2	0			0	9
	3	0			0	3
No without previous experience on same problem.....			0	2	0	31
			—	—	—	—
	1	1	0	1		
	—	1	0	1		
No with previous experience on same problem..			0	2		
			—	—		
Blank	0	0	0	1	1	0
			=	=	=	=
Total			6	31	13	76

it. Estimates of p_I, therefore, may be biased by both previous *full* experience and "aspiration" to solve.

When individuals are assigned to solve the Tartaglia problem in groups, there must be variation in the composition of the groups, not only by aspiration but also by extent of previous experience with a similar or the same problem. Therefore, Tables 4 through 8 are presented giving group results classified by the number of Yes's to question 7 and by a score for extent of previous experience. This score is a rough one and not entirely an adequate measure of amount of experience which is obtained by summing the scores of the individuals within the groups, first for similar problems and then for the same problem. For example, experience on the same problem reported as 0 by one individual, 1 by another, and 2 by a third, while summing to three and scored that way, is not

TABLE 5

SOLUTIONS TO TARTAGLIA PROBLEM BY THREE-MEMBER GROUPS: CLASSIFIED BY THE NUMBER OF PERSONS HAVING ASPIRATION TO SOLVE AND BY THE NUMBERED SCORE FOR AMOUNTS OF PREVIOUS EXPERIENCE WITH A SIMILAR OR THE SAME PROBLEM

Number of Yes's to Question 7 out of 3 in group	Sum of Yes's to Previous Experience		Solution	Fail
	Similar	Same		
1	0	0		1
	2	0		2
	5	0		3
	7	0		1
			Sum 0	7
2	0	0		1
	3	0	1	
	4	0		2
	5	0		2
	6	0		1
			Sum 1	6
3	4	0		1
	5	0	2	
	6	0		1
			Sum 2	2
3	3	1		1
	8	2		1
	3	3		1 1 at 3/3Y*
			Sum 0	3
0	0	0		1
	6	2		1
			Sum 0	2
Total			3	20

* In this group at least one person claimed to have solved the problem previously.

equivalent to the three of claimed solution by an individual.

There were 24 groups containing at least one member who claimed to have solved the Tartaglia problem previously. Of these 24 groups, ten solved the problem, i.e., 42 per cent. The groups classified by group size together with the number of solutions and failures is for

Groups of three,	no solution—one failure
four,	three solutions—two failures
six,	two solutions—five failures
seven,	five solutions—six failures

Partial solution can also be employed to check on the tenability of the model (1).

TABLE 6

SOLUTIONS TARTAGLIA PROBLEM BY FOUR-MEMBER GROUPS: CLASSIFIED BY THE NUMBER OF PERSONS HAVING ASPIRATION TO SOLVE AND BY THE NUMBERED SCORE FOR AMOUNTS OF PREVIOUS EXPERIENCE WITH A SIMILAR OR THE SAME PROBLEM

Number of Yes's to Question 7 out of 4 in group	Sum of Yes's to Previous Experience		Solution	Fail	
	Similar	Same			
1	5	0	1		
2	2	0		1	
3	6	0	1		
	7	0	1		
	9	0	1		
4	7	0	1		
	10	0		1	
Yes without previous experience on same problem.....			–	–	
			5	2	
2	4	2	1	1	
	4	3	1		
3	7	6	1		1 at 1/3Y at 3/3Y*
4	7	3		2	1 each at 3/3Y*
	8	3	1		1 at 3/3Y*
	10	3	1		1 at 3/3Y*
Yes with previous experience on same problem.....			–	–	
			5	3	
Total			10	5	

* In this group at least one person claimed to have solved the problem previously.

TABLE 7

SOLUTIONS TO TARTAGLIA PROBLEM BY SIX-MEMBER GROUPS: CLASSIFIED BY THE NUMBER OF PERSONS HAVING ASPIRATION TO SOLVE AND BY THE NUMBERED SCORES FOR AMOUNTS OF PREVIOUS EXPERIENCE WITH A SIMILAR OR THE SAME PROBLEM

Number of Yes's to Question 7 out of 6 in group	Similar	Same	Solution	Fail	
2	6	0		1	
	8	0		1	
	13	0	1		
3	2	0		1	
	5	0		1	
	6	0		1	
4	6	0		1	
6	6	0	1		
	7	0		1	
	12	0		1	
	14	0		1	
Yes without previous experience on same problem.....			–	–	
			2	9	
1	14	2		1	
3	9	2		1	1 at 3/3Y*
	8	3	1	1	1 at 3/3Y*, 1 at 3/3Y*
5	8	2		1	
	12	4		1	
	12	5		1	
	13	5		1	1 at 3/3Y*
	14	6	1	1	2 at 3/3Y*, 1 at 3/3Y*
6	12	3		1	1 at 3/3Y*
Yes with previous experience on same problem.....			–	–	
			2	9	
Total			4	18	

* In this group at least one person claimed to have solved the problem previously.

For example, since the Tartaglia requires successful passage through several interrelated steps, then larger group sizes should get farther in partial solutions. For individuals and groups who did not solve the problem successfully, the step at which each individual and group failed was recorded. The solutions and the steps in arriving at the solution are based on an analysis of the problem as a problem and not on the experimental results. The solutions by steps (which

TABLE 8

SOLUTIONS TO TARTAGLIA PROBLEM BY SEVEN-MEMBER GROUPS: CLASSIFIED BY THE NUMBER OF PERSONS HAVING ASPIRATION TO SOLVE AND BY THE NUMBERED SCORE FOR AMOUNTS OF PREVIOUS EXPERIENCE WITH A SIMILAR OR THE SAME PROBLEM

Number of Yes's to Question 7 out of 7 in group	Similar	Same	Solution	Fail
5	6	0	1	0
6	8	0		1
7	7	0	1	
	8	0		1
Number of Yes's without previous experience on same problem.....			– 2	– 2
4	11	3	1	1 at 3/3Y*
5	11	3	1	1 at 3/3Y*
	13	6		1 1 at 3/3Y*
6	8	3		1 1 at 0/3N*
	6	5		1 1 at 0/3Y*
	17	6		1 1 at 3/3Y*
				1 at 3/3N*
7	3	3	1	1 at 3/3Y*
	13	3	1	1 at 3/3Y*
	10	4		1
	16	4	1	1 at 3/3Y*
	12	12		1 3 at 3/3Y*
Number of Yes's with previous experience on same problem.....			– 5	– 6
Total			7	8

* In this group at least one person claimed to have solved the problem previously.

may, of course, be worded somewhat differently) are:

A

Step 1) H1 and W1 cross over to B-side
Step 2) W1 remains on B-side. H1 returns to A-side
Step 3) H1, H2, and W2 cross over to B-side
Step 4) H1 and W1 remain on B-side, H2 and W2 return to A-side
Step 5) H2, W3, and H3 cross over to B-side
Step 6) H3 and W3 remain on B-side, H2 returns to A-side
Step 7) H2 and W2 cross over to B-side

B

Step 1) H1 and W1 cross over to B-side
Step 2) W1 remains on B-side, H1 returns to A-side
Step 3) H1, H2, and H3 cross over to B-side
Step 4) H1 remains on B-side, H2 and H3 return to A-side
Step 5) H2, H3, and W2 cross over to B-side
Step 6) H3 returns to A-side
Step 7) H3 and W3 cross over to B-side

If a subject or group failed to begin the problem or specified the problem as beyond their ability, or the paper was turned in blank, or the solution was crossed out, then the failure was designated as a Step 0 failure. Step 0, therefore, indicates complete failures in contrast to efforts which give attempts at Step 1 which are unsuccessful. For those who failed, the step at which the failure was made

TABLE 9

GROUP PERFORMANCE AND "LEVEL OF ASPIRATION"

Number of No's in group	Solutions	No Solutions	PG
	Groups of 3		
3	0	2	0
2	0	7	0
1	1	6	.14
0	2	5	.28
	Groups of 4		
4	—	—	
3	1	0	1
2	2	2	.50
1	4	0	1
0	3	3	.50
	Groups of 6		
6	—	—	
5	0	1	0
4	1	2	.33
3	1	5	.17
2	0	1	0
1	1	5	.17
0	1	4	.20
	Groups of 7		
7	—	—	
6	—	—	
5	—	—	
4	—	—	
3	1	0	1
2	2	0	1
1	0	5	0
0	4	3	.56

was reported but no definitive relation was obtained. These data are not reported in Tables 4 through 8 but are available upon request.

There also does not appear to be a definitive relation between prior exposure and successful solution. In Table 4, for example, contrasting solutions by individuals in Class 1 with scores 3-0 and better against the remainder, it is found that 5 out of 34 of the former and 6 out of 44 of the latter solved successfully, i.e., proportions of .147 and .120, respectively. For all individuals 11 out of 89 solve, i.e., a proportion of .124. Thus, there may be a slight but tenuous tendency that individuals with much prior exposure do somewhat better. The same table suggests similar findings on individual performance for Class 2; 2 solutions out of 11 attempts for scores 3-0 and better, 4 out of 26 for the remainder giving proportions of .182 and .154; for all individuals, 6 out of 37 for a proportion of .165.

For groups of four, six, and seven it is difficult to score prior exposure since practically all groups have at least one person with a score of 3-0 or better. In the tables for these three situations, some attempt is made at scoring the group from least exposed to most exposed but this results in a uniform distribution of successful solutions along the exposure dimension.

In groups of three, 10 out of 23 groups did not contain an individual with a Similar-Same Score as high as 3-0. One out of these ten and two out of the remaining 13 solved the problem yielding proportions of .100 and .154. Moreover, in this situation as contrasted with the other group situations, the data allow a better determination of the influence, if any, of prior exposure since the dichotomy placed approximately half the observations in each category.

When we consider "level of aspiration" as a sole predictor variable there is a more definite relationship with successful performance. As mentioned before, no individual, working as an individual, who indicated he or she could not solve the problem actually solved the problem. Table 9 shows the effect of individual "level of aspiration" on group performance in groups of three, four, six, and seven. Here we see some evidence of better group performance as the group contains more individuals who conjecture they can solve the problem.

VARIANCE OF $\hat{p}_G$

Since knowledge of the variance of $\hat{p}_G$, $\sigma^2_{\hat{p}_G}$, (the proportion of group solutions predicted by the model from knowledge of p_I) would be helpful in choosing among several explanations of group performance, we shall develop it in this section. (For simplicity, the caret is omitted from $\hat{p}_G$ in what follows.)

$$\sigma^2_{p_G} = Ep^2_G - (Ep_G)^2$$

$$\sigma^2_{p_G} = \sigma^2_{1-p_G} = E(1-p_G)^2 - [E(1-p_G)]^2 \tag{1}$$

where E denotes expected value. Now, by the model,

$$\begin{aligned}(1 - p_G) &= [(1 - P_I) - (p_I - P_I)]^k \\ &= (1 - P_I)^k - k(1 - P_I)^{k-1}(p_I - P_I) \\ &\quad + \frac{k(k-1)}{2}(1 - P_I)^{k-2}(p_I - P_I)^2 \\ &\quad - \frac{k(k-1)(k-2)}{6} \\ &\quad \cdot(1 - P_I)^{k-3}(p_I - P_I)^3 \\ &\quad + \frac{k(k-1)(k-2)(k-3)}{24} \\ &\quad \cdot(1 - P_I)^{k-4}(p_I - P_I)^4 \cdots\end{aligned} \tag{2}$$

Thus, since $E(p_I - P_I)^j$ is the j^{th} central moment of the binomial distribution and is known, we get

$$\begin{aligned}E(1 - p_G) &= (1 - P_I)^k + \frac{k(k-1)}{2} \\ &\quad \cdot(1 - P_I)^{k-2}\frac{P_I(1 - P_I)}{N_I} \\ &\quad - \frac{k(k-1)(k-2)}{6}(1 - P_I)^{k-3} \\ &\quad \cdot\frac{P_I(1 - P_I)(1 - 2P_I)}{N_I^2} \\ &\quad + \frac{k(k-1)(k-2)(k-3)}{24} \\ &\quad \cdot(1 - P_I)^{k-4}\left[\frac{3P_I^2(1 - P_I)^2}{N_I^2}\right. \\ &\quad \left. + \frac{P_I(1 - P_I)(1 - 6P_I + 6P_I^2)}{N_I^3}\right] \\ &\quad + O\left(\frac{1}{N_I}\right)^3.\end{aligned} \tag{3}$$

TABLE 10

VALUES OF A AND B IN $\sigma^2_{p_G} \sim A(1/N_I) + B(1/N_I^2)$; WHERE P_I IS PROPORTION OF SUCCESSFUL SOLUTIONS BY INDIVIDUALS AND THERE ARE k INDIVIDUALS IN A GROUP; FIRST ENTRY IN CELL IS A; SECOND ENTRY IN CELL IS B

P_I	k						
	2	3	4	5	6	7	10
.05	.1715	.3482	.5587	.7878	1.0238	1.2577	1.8868
	−.1579	−.5864	−1.2791	−2.1561	−3.0985	−3.9716	4.9155
.10	.2916	.5314	.7653	.9686	1.1297	1.2455	1.3509
	−.2430	−.7086	−1.1479	−1.2914	−.9414	0	6.0788
.15	.3685	.5990	.7694	.8686	.9037	.8887	.6840
	−.2709	−.5638	−.4752	.2044	1.4618	3.1364	8.5097
.20	.4096	.5893	.6711	.6711	.6185	.5388	.2882
	−.2560	−.2949	.0839	1.3422	2.7058	4.0407	6.1609
.25	.4219	.5339	.5339	.4693	.3801	.2910	.1057
	−.2109	0	.8009	1.8771	2.8509	3.4923	3.3615
.30	.4116	.4538	.3953	.3027	.2136	.1424	.0342
	−.1470	.2593	1.1012	1.9024	2.3643	2.4416	1.4729
.35	.3845	.3655	.2745	.1812	.1103	.0634	.0098
	−.0739	.4498	1.1720	1.6171	1.6751	1.4632	.5506
.40	.3456	.2799	.1792	.1008	.0522	.0256	.0024
	0	.5599	1.0750	1.2093	1.0448	.7680	.1755
.45	.2995	.2038	.1096	.0518	.0226	.0093	.0005
	.0681	.5930	.8819	.8101	.5796	.3548	.0475
.50	.2500	.1406	.0625	.0244	.0088	.0030	.0001
	.1250	.5625	.6563	.4883	.2856	.1436	.0107

Therefore,

$$[E(1-P_G)]^2 = (1-P_I)^{2k} + k(k-1)(1-P_I)^{2k-2}\frac{P_I(1-P_I)}{N_I} + \frac{1}{N_I^2}\left\{\frac{k(k-1)(k^2-3k+2)}{2}\cdot(1-P_I)^{2k-4}P_I^2(1-P_I)^2 \cdot\frac{-k(k-1)(k-2)}{3}(1-P_I)^{2k-3}\cdot P_I(1-P_I)(1-2P_I)\right\} + O\left(\frac{1}{N_I}\right)^3. \quad (4)$$

Now

$$(1-p_G)^2 = [(1-P_I) - (p_I - P_I)]^{2k}. \quad (5)$$

Taking the expected value of both sides, we get

$$E[(1-p_G)^2] = (1-P_I)^{2k} + \frac{2k(2k-1)}{2}(1-P_I)^{2k-2}\cdot\frac{P_I(1-P_I)}{N_I} - \frac{2k(2k-1)(2k-2)}{6}\cdot\frac{(1-P_I)^{2k-3}P_I(1-P_I)(1-2P_I)}{N_I^2} + \frac{2k(2k-1)(2k-2)(2k-3)}{24}\cdot(1-P_I)^{2k-4}\left[\frac{3P_I^2(1-P_I)^2}{N_I^2} + \frac{P_I(1-P_I)(1-6P_I+6P_I^2)}{N_I^3}\right] + O\left(\frac{1}{N_I}\right)^3. \quad (6)$$

Thus

$$\sigma^2_{p_G} = E[(1-p_G)^2] - [E(1-p_G)]^2 = \frac{P_I(1-P_I)}{N_I}\left\{\left[\frac{2k(2k-1)}{2} - k(k-1)\right](1-P_I)^{2k-2}\right\} + \frac{P_I^2(1-P_I)^2}{N_I^2}\cdot\left\{\left[\frac{2k(2k-1)(2k-2)(2k-3)}{8} - \frac{k(k-1)(k^2-3k+3)}{2}\right]\cdot(1-P_I)^{2k-4}\right\} + \frac{P_I(1-P_I)(1-2P_I)}{N_I^2}\cdot\left\{\left[\frac{k(k-1)(k-2)}{3} - \frac{2k(2k-1)(2k-2)}{6}\right](1-P_I)^{2k-3}\right\} + O\left(\frac{1}{N_I}\right)^3. \quad (7)$$

$$\sigma^2_{PG} = \frac{P_I(1-P_I)}{N_I}[k^2(1-P_I)^{2k-2}] + \frac{P_I^2(1-P_I)^2}{N_I^2} \cdot \left\{\frac{k^2(k-1)(3k-5)}{2}(1-P_I)^{2k-4}\right\} - \frac{P_I(1-P_I)(1-2P_I)}{N_I^2} \cdot \{k^2(k-1)(1-P_I)^{2k-3}\} + O\left(\frac{1}{N_I}\right)^3 \quad (8)$$

or

$$\sigma^2_{PG} = A\frac{1}{N_I} + B\frac{1}{N_I^2} + O\left(\frac{1}{N_I}\right)^3$$

where

$$A = k^2P_I(1-P_I)^{2k-1}$$

$$B = k^2(k-1)P_I(1-P_I)^{2k-2} \cdot [\tfrac{1}{2}P_I(3k-1)-1]. \quad (9)$$

TABLE 11

MAXIMUM VALUES OF A IN $\sigma^2_{PG} \sim A(1/N_I) + B(1/N_I^2)$ FOR FIXED k AND CORRESPONDING P_I AND FOR FIXED P_I AND CORRESPONDING k

Fixed k	Corresponding P_I	A
2	.2500	.4219
3	.1667	.6026
4	.1250	.7854
5	.1000	.9686
6	.0833	1.1529
7	.0714	1.3367
8	.0625	1.5192
9	.0556	1.7052
10	.0500	1.8868

Fixed P_I	Corresponding k*	A
.05	19.5	2.7060
.10	9.5	1.3530
.15	6.2	.9032
.20	4.5	.6786
.25	3.5	.5444
.30	2.8	.4561
.35	2.3	.3926
.40	2.0	.3458
.45	1.7	.3098
.50	1.4	.2816

* k must be an integer but is given to the nearest tenth to point up differences.

Table 10 gives A and B for k = 2, 3, 4, 5, 6, 7, the range of experimental group sizes and for k = 10, a possible upper limit to the size of groups employed to solve Eureka type puzzles; and for P_I = .05 to P_I = .50 at intervals of .05 since P_I = .50 seems a realistic upper bound beyond which groups probably would not be formed to attempt solution. One interesting point to observe is that maxima occur for A for fixed k, and for fixed P_I, and A is the important contribution to σ^2_{PG}. Thus within the range of experimental values of group size and P_I, it is possible to get dramatic changes in σ^2_{PG} by small changes in P_I and group size. Table 11 gives the maximum value of A and resulting value of P_I for fixed k and the maximum value of A and resulting value of k for fixed P_I.

SUMMARY

There can be little doubt that "level of aspiration" significantly affects the number of solutions to the problem given. Whether "aspiration" reflects a genuine self-report of lack of ability, or lack of interest, or lack of effort cannot be ascertained from the data, but it is significant that no individual without aspiration, working alone, was able to solve the problem. To some degree, the tendency may be carried over with the performances of groups. The data, however, are not only sparse but they are contaminated by the amount of previous knowledge the individuals brought to the solution. Among groups, if at least a member claimed to have solved the same problem previously, the relative number of solutions in general supported the assumption underlying the model, i.e., if the group has at least a member with the ability to solve, the problem will be solved. Since the validity of the retrospective memory cannot be evaluated, some members may have overclaimed (or underclaimed) their previous experience with the Tartaglia problem. Taking the claims at face value, however, suggests that there may be instances in which the member with the ability to solve does not contribute or may be prevented from contributing his skill.

In problem-solving experiments, there is a need to consider the factor of previous experience. In the classes participating in this

experiment, about a third indicated that they had no previous experience with a similar or the same problem, that is, about two thirds had some experience either with a similar problem or with the same problem. While it cannot be expected that previous experience will inevitably transfer or that the experience when it transfers will transfer positively, there is some chance that previous experience may be one of the factors influencing the reported differences in the proportion of solutions by different groups at different times. In comparing the results of experiments at different time periods, researchers should be aware of the difficulty in assuming equivalence of populations. Perhaps the variance of proportions of group solutions developed in this paper under the assumption that the ability model is operating may be helpful in choosing among several explanations of group performance.

REFERENCES

Kasner, E., and Newman, J. *Mathematics and the imagination*. New York: Simon and Schuster, 1940.

Lorge I., and Solomon, H. Two models of group behavior in the solution of Eureka-type problems. *Psychometrika*, 1955, 20, 139–148.

Lorge I., and Solomon, H. Individual performance and group performance in problem solving related to group size and previous exposure to the problem. *J. soc. Psychol.*, 1959, 48, 107–114.

MARQUART, D. I. Group problem solving. *J. soc. Psychol.*, 1955, 41, 103–113.

Shaw, M. E. Comparison of individuals and small groups in the rational solution of complex problems. *Amer. J. Psychol.*, 1932, 44, 491–504.

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A certain man of Cheng was going to buy himself a new pair of shoes. He took measurements of his feet, but he forgot to bring the measurements along when he went to a shoe shop. He returned home for the measurements but the shop was closed by the time he went back to it so he could not buy any shoes. Someone asked him, "Why didn't you let them try the shoes on your feet?" And the man replied, "I would rather trust the measurements than trust myself."

HAN FEI-TSE (*tr. Lin Yutang*)

Problem solving is here viewed as a process in which a person constructs a population of possible hypotheses about his environment, selects one or a few of these for testing, submits them to a test, receives information from the environment regarding the truth or falsity of the hypotheses tested, adjusts his population of possible hypotheses in the light of the available evidence, selects a new hypothesis, and so on, until he has created some "fit" between his guesses about the environment and his experience in it. The question raised in this paper is whether among the "population" of hypotheses available to the subject, certain ones will be given preference and so chosen before the others. It seems such biases do operate and can be detected.

AN INVESTIGATION INTO, AND SPECULATIONS ABOUT, THE FORMAL NATURE OF A PROBLEM-SOLVING PROCESS

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MACHINES built to solve problems have been with us for some time now and many more are bound to come. Of note, for example, are the work of Newell, Shaw, and Simon (1958) and the artefacts proposed by MacKay (1956). Of interest to psychologists would be machines that solve problems as humans do. This requires first some research on humans solving problems, expressing the process in a language which is capable of translation into a program. It is hoped that the present study contributes toward this aim.

The focus of attention in this study is centered on problem solving *as a process* and not on any ultimate outcomes of the process, successful or otherwise. What is of interest is the succession of trials made by the subject in solving a problem, the information available to him prior to each trial, and the "mechanism" inside the subject which might account for the specific linkage between information (input) and trial (output) which can be observed.

It has been found convenient to think of intelligent behavior as an *information flow system* in which certain transactions take place between the person and an environment. In the situation studied, the subject, when given a problem to solve, makes a given trial, receives information from the environment (i.e., from the problem), then makes a further trial, and so on until he receives a signal from the environment which conforms to his requirement for problem solution. A schematic representation of intelligent behavior as proposed by MacKay (1956) is shown in Figure 1. F is the environment or "Field." The subject is composed of the three linked subsystems labelled C, E, and R. E is the person's 'effector system,' i.e., the system which performs certain trials on the problem. E is governed by C, or the 'control system.' This is the person's cognitive or control system which scans from moment to moment among the possible action alternatives which have not as yet been explored and which selects what E shall do next. C must receive information from the field concerning the success or failure of E's trials and therefore concerning the system's relative closeness to the goal (as indicated by the length of the interval X-Y). This information it receives via a receptor system, R.

For the experiments which will be described here a task was available such that the trials made by the subject from

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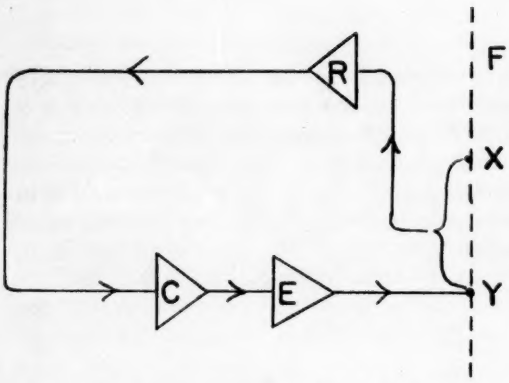

FIG. 1. Schematic representation of intelligent behavior.

moment to moment (i.e., the outputs of the effector system, *E*), the information forthcoming from the environment (i.e., the inputs to the receptor system, *R*), and the achievement of the goal (i.e., the coincidence of *X* and *Y*) could be measured objectively. With the help of these measures it was then possible to test certain notions concerning the *processes* of selecting among specific effector actions going on inside the control system *C*. That is, it was possible to test certain hypotheses about the thinking process. Granted that there is a control system, at least three questions may be asked with regard to its modes of operation.

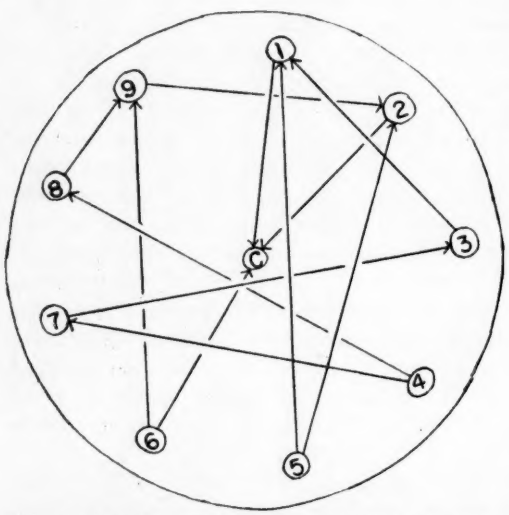

FIG. 2. Chart indicating among which lights relations exist.

1. What is the *population* of alternative events considered by the control system at any given moment? Is its formulation of events exhaustive or restricted? If the latter, what seem to be the principles generating restriction?

2. Given the population of alternative events, does the control system scan randomly among these in selecting its input to the effector system? If not, can certain biases be pinpointed and quantified?

3. Does the population of alternative events remain constant (in the sense that no *new* alternatives are added) from moment to moment or do new alternative events, previously not incorporated, become included as a function of experience?

Various theoretical models which deal with these three questions concerning the control system will be outlined. However, since a descripion of the models can be given most concretely in regard to the experimental task given the subjects, a description of this task is in order first. Descriptions of the experimental task have appeared in a number of publications (John, 1957; John and Miller, 1957; and Rapoport, 1958). The statements concerning the experimental task which follow below have been taken largely from Rapoport.[2]

"The subject is faced with a board on which nine numbered light bulbs are arranged in a circle at the center of which is a tenth bulb. Each of the peripheral bulbs may be lit by an adjacent button. Moreover, relays are so arranged inside the apparatus that lighting of certain lights may result in the lighting of other lights following a constant 'synaptic delay' of three seconds. A 'problem' is a programming in the apparatus so that certain causal relations are *only partially* represented by arrows on a chart attached to the mounting board. An example is given in Figure 2.

The point of the problem is that the mean-

[2] The apparatus to be described, the PSI, based on the isomorphism of certain networks of relays and the calculus of propositions [previously discovered by Shannon (1938), and by McCulloch and Pitts (1943)] was developed in Chicago by R. John, J. G. Miller, S. Molnar, and H. J. A. Rimoldi.

ing of the arrows on the chart is ambiguous. An arrow from 1 to the center light C may mean that 1 is necessary to light C or sufficient, or both, or that 1 inhibits C. The subject's task is to obtain sufficient information about these relations, by pushing any button he chooses, to be able to cause the center bulb to be lit by manipulating buttons 4, 5, and 6 only. We will refer to these as the circled buttons.

There is a unique solution to each problem, consisting of a certain sequence of pushes of the circled buttons or of their combinations. For example, the solution to the problem shown in Figure 2 is the pushing of buttons 4, 6, 5, 6 in the successive time periods.

There are a number of 'rational' approaches to the problem. Let us begin by making a chart of the connections indicated by the arrows. Figure 2 is formally equivalent (as a linear graph) to Figure 3. However, many characteristics of the problem are visually displayed in Figure 3, which immediately suggest various lines of attack. This chart displays what the subject does not know. For example, in Figure 3, the convergence of arrows leading from 1, 2, and 6 upon C could induce the following questions: Which combinations of 1, 2, and 6 are necessary or sufficient or both to light C? Is one or more of them an inhibitor? If so, which one? Similar ambiguities are apparent in the other three junctures, that is at lights 1, 2, and 9. A single arrow leading to a light presents no ambiguity. This is in consequence of the condition explained to the subject that each arrow 'means' something, hence a single arrow can mean only 'necessary and sufficient,' otherwise its presence or absence would make no difference."

Thus, given that the subject formulates the problem in terms of a chart as represented in Figure 3, he will ask himself some questions concerning the ambiguities present at each of the four junctures. Most subjects start with the ambiguities which are apparent at C, and then work "backward" to the ambiguities existing at lights 1, 2, and 9, respectively. It is at this point that some theoretical models are needed if an attempt to predict the various consecutive choices which subjects will make over a number of trials is to have a rationale. These theoretical models must deal with the questions concerning the processes of the control system which were listed earlier: (a) what is the population of alternative events, or the population of questions, with which the subject is concerned from trial to trial; (b) does the control system sample randomly within these or, if not, can its biases be identified and quantified; and (c) does the population of alternative events remain constant from moment to moment or do new alternative events, previously not considered, become included as a result of experience? Three alternative theoretical models will now be advanced, demonstrating that more than one answer can be given to the three questions which have been raised. An empirical test of each model will then help to answer the question of how the control system operates or at least how it does not.

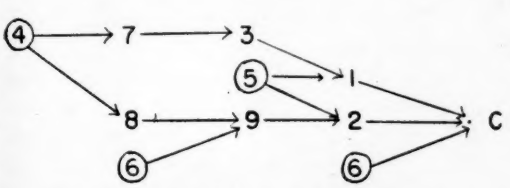

FIG. 3. Fig. 2 displayed in time sequence.

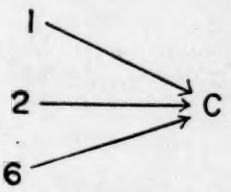

FIG. 4. Graph depicting ambiguities existing with respect to the convergence of 1, 2, and 6 on C.

The models will only be carried to an analysis of the ambiguities existing with respect to lights C, 1, and 2. However, in the discussion section, the application of the models to the entire problem-solving process will be considered.

MODEL 1

In Model 1 the following psychological events are postulated:

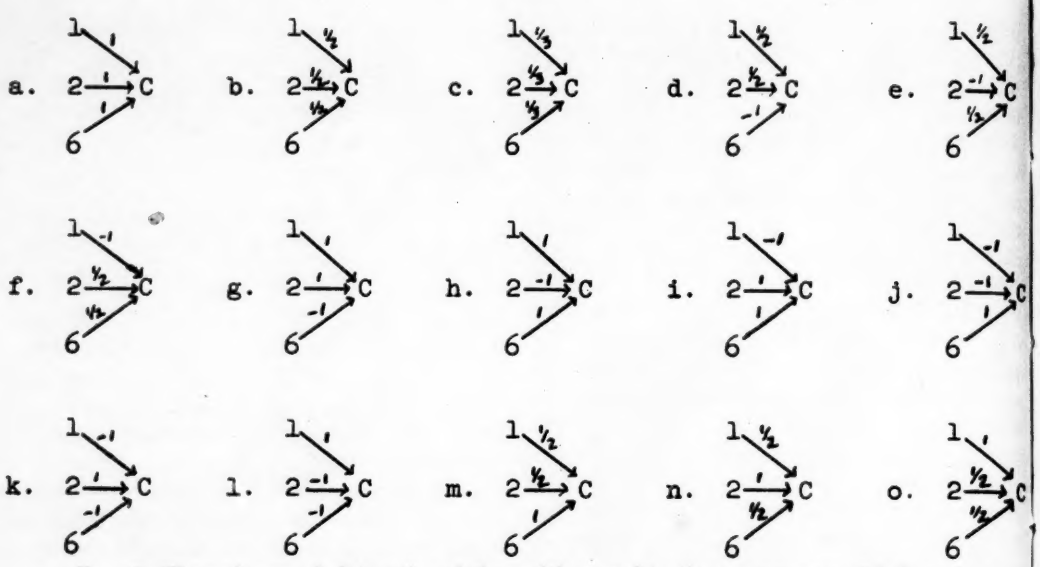

FIG. 5[3]. The universe of alternative choices with regard to the convergence of 1, 2, and 6 on C on the first trial which is made in respect to that convergence.

1. **Goals.** Having been given the experimental task, the subject breaks the total goal (i.e., "to light the center light using circled buttons only") up into a number of subgoals, the most immediate of which is to light the center light from the lights directly impinging on it.

2. **The universe of alternative choices.** Having formulated the goal, the subject then considers the question what combinations of 1, 2, and 6 could light *C*. In Model 1 it is assumed that the universe of possible combinations is made up of all the graphs which it is possible to construct by assigning permitted values to the arrows in the graph given in Figure 4. That is, the subject perceives the graph in Figure 4 *as a whole* and proceeds, by assigning meanings or values to the arrows, to list various meanings which the ambiguous graph in Figure 4 might have. The universe of graphs obtained as a result of the assignment of all possible values to the three arrows is given in Figure 5. The subject does not necessarily consider the entire universe of graphs prior to a given trial. All that is assumed for the moment in Model 1 is that the subject proceeds from a consideration of Figure 4 as a whole, that he scans or samples randomly within this universe considering various alternatives, and that at a given time (which may or may not be random) during this scanning process he decides to try the alternative under consideration at that moment. Given a random scanning process and selection of a given alternative at a given time, the probability that a certain alternative will be chosen can be derived from a knowledge of the universe of alternatives (see Figure 5). Since the subject's endeavor is to find out how to light the center light, the alternatives with which he will be concerned will be:

a. 1 lights *C*
b. 2 lights *C*
c. 6 lights *C*
d. 1 and 2 light *C*
e. 1 and 6 light *C*
f. 2 and 6 light *C*
g. 1, and 2, and 6 light *C*

[3] A 1 over an arrow denotes a sufficiency. Thus, in alternative *a*, all three lights 1, 2, and 6 are in themselves sufficient to light *C*. A fraction over an arrow indicates a necessity but not a sufficiency. If the fraction is ½, it means that the light from which the given arrow emanates *plus* one other light will activate *C*. Thus, in alternative *b*, it is indicated that combinations 1, 2, and 1, 6, and 2, 6 will all light *C*. A minus 1 over an arrow indicates an inhibition. Thus, in alternative *d* the combination of 1 and 2 will light *C* but, as a result of the inhibitory capacity of 6, should 1, 2, *and* 6 be pressed together, *C* would not light.

In Table 1, column 2 are presented the probabilities of each of these events occurring given the above universe, and hence the probabilities of each of these events being *selected* as a trial. In column 3 of Table 1 are listed the probabilities that a *class* of choices will be made. For example, the class of choices labeled $w \xrightarrow{1} z$ includes choices $1 \xrightarrow{1} C$, $2 \xrightarrow{1} C$, $6 \xrightarrow{1} C$. The probability that a choice of the class $w \xrightarrow{1} z$ will be made is now the probability that the subject will make a $1 \xrightarrow{1} C$, or a $2 \xrightarrow{1} C$, or a $6 \xrightarrow{1} C$ choice. Given a group of subjects, the probabilities in Table 1 can be used to predict the percentage, or the number, of subjects which might be expected to make a choice of the class $w \xrightarrow{1} z$, of the class $w,x \xrightarrow{1} z$, or of the class $w, x,y \xrightarrow{1} z$.

After a choice has been made, certain of the possible alternatives in the universe (see Figure 5) become eliminated. New probabilities for the next choice can then be computed from this more limited universe in the same way in which they were computed for the original universe (see Table 1).

The assumption so far has been that scanning within the universe of alternate choices occurs on a random basis. However, at least two biases in the scanning process can conceivably be postulated. One is what will be called a bias toward *simplicity*, the other is a bias toward *strategy completion or persistence* in a given strategy. The first bias takes the form of scanning consistently more heavily than is warranted by frequency of occurrence in the population those alternatives which are simple than alternatives which are more complex. It has been assumed that alternatives of the class $w \xrightarrow{1} z$ are simpler than those of the class $w,x \xrightarrow{1} z$, or $w,x,y \xrightarrow{1} z$. The second bias takes the form of a tendency to continue to test class $w,x \xrightarrow{1} z$ choices if a class $w,x \xrightarrow{1} z$ choice has been made just prior to the present choice. This tendency too, therefore, leads to the scanning of a certain class of alternatives more heavily than is warranted by the frequency of their occurrence in the universe. These two biases can be quantified by comparing predictions based on random scanning with actual choices made by subjects. Each bias can then be expressed as a constant and used to modify the probability values obtained under the assumption of random scanning.

TABLE 1

THE PROBABILITY THAT CHOICES OF A GIVEN CLASS WILL BE MADE BY A GROUP OF SUBJECTS IN RESPONSE TO THE CONVERGENCE OF 1, 2, AND 6 ON C, ON THE FIRST TRIAL IN RESPECT TO THAT CONVERGENCE

	Probability	
$1 \xrightarrow{1} C$	5/42	
$2 \xrightarrow{1} C$	5/42	$W \xrightarrow{1} Z$: 15/42 = .36
$6 \xrightarrow{1} C$	5/42	
1, 2 → C	7/42	
1, 6 → C	7/42	W, X → Z: 21/42 = .50
2, 6 → C	7/42	
1, 2 → C	6/42	W, X → Z: 6/42 = .14, Y

The description of Model 1 is now complete. The theoretical questions concerning the control process have been dealt with, i.e., (a) the universe of alternative choices has been outlined, and (b) it has been shown how certain biases in a random scanning process might be identified and quantified.

Model 1 may now be applied to the next set of major steps in the problem solving sequence. These concern the trials which the subject makes after he has been successful in lighting C by trying 1, 2, or 6 singly or in combinations. In this particular problem the subject will have found out that the combination of 1 and 6 lights the

FIG. 6. Graph depicting ambiguities existing with respect to the convergence of 3 and 5 on 1.

center. If the subject has tried one other pair, in addition to 1 and 6, he will further know that 2 inhibits the center. To apply Model 1 the goal or goals and the universe of means must again be stated.

1. Goal(s). Assume that the subject knows that the combination of 1 and 6 lights C and that 2 inhibits C. The subject's most immediate goal now might become just simply to light 1 from the lights directly impinging on it, or to light 1 *without* lighting 2. In the latter case he would not merely be concerned with the lights which impinge on 1 but also with whether they impinge on 2 and therefore might activate 2, creating a combination of 1 and 2 when the activation of 1 alone is desired. On a priori grounds there seems to be no reason to favor one assumption over the other. Thus each assumption will be allowed and its consequences tested against the experimental results. Either of the following two assumptions regarding goals may be made:

a. (The weaker assumption). The subject's goal is to light 1 from the lights directly impinging on 1.

b. (The stronger assumption). The subject's goal is to light 1 from the lights impinging on 1 *and* to avoid 2.

2. Universe of alternative choices.

a. If assumption *a* (the goal is: to light 1) is made, the subject will be interested in the lights which impinge on 1 and he will be concerned with the total graph shown in Figure 6. The universe of alternative choices is given in Figure 7.

The probability with which the classes of alternative choices $w \xrightarrow{1} z$, and $w,x \xrightarrow{1} z$ will occur in a number of subjects is given in Table 2.

Probabilities for consecutive trials may again be computed by taking account of alternatives eliminated by a given trial and computing probabilities using a more restricted universe as a basis.

The class of alternative choices $w \xrightarrow{1} z$ is of course made up in this instance of the choice $3 \xrightarrow{1} 1$ or the choice $5 \xrightarrow{1} 1$. If the former is chosen by the subject, he finds that 3 does not light 1 which still leaves him with the alternatives *c* and *d* (see Figure 7). If the subject has chosen $5 \xrightarrow{1} 1$, he finds that 5 does not light 1 but that 5 lights 2 (which he knows must be avoided). Inasmuch as for this subject alternatives *b* and *d* remain (See Figure 5) and inasmuch as alternative *d* includes 5 (and therefore 2), it is assumed that under these circumstances the subject no longer can restrict his attention to the lighting of 1 solely, but must include in his goal the avoidance or inhibition of 2. This case then automatically reverts to what was called assumption b regarding the subject's goal (i.e., "to light 1 without 2").

b. If assumption b is made regarding the subject's goal he will from the outset be interested in the lights which impinge on 1 as well as in the lights which impinge on 1 and 2, if any, and if the latter is the case, he will be interested in the lights which impinge on 2 generally (inasmuch as he must take account of the fact that it may not be possible to avoid 2 in the lighting of 1 and consequently that it may be necessary to inhibit it). Thus the subject will be concerned with the total graph shown in Figure 8.

The alternative choices with which the subject will be concerned are listed in Figure 9.

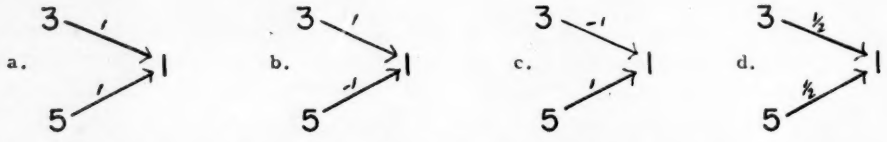

FIG. 7. The universe of alternative choices with regard to the convergence of 3 and 5 on 1, on the first trial which is made in respect to that convergence.

TABLE 2

THE PROBABILITY THAT CHOICES OF A GIVEN CLASS WILL BE MADE BY A GROUP OF SUBJECTS IN RESPONSE TO THE CONVERGENCE OF 3 AND 5 ON 1, ON THE FIRST TRIAL IN RESPECT TO THAT CONVERGENCE

Classes of alternative choices	Probability
$W \xrightarrow{1} Z$	4/6 = .67
$W,X \xrightarrow{1} Z$	2/6 = .33

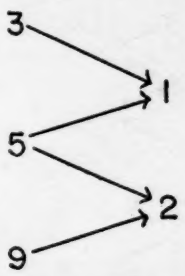

FIG. 8. Graph depicting ambiguities existing with respect to the convergence of 3 and 5 on 1, and of 5 and 9 on 2.

In Table 3 are listed the probabilities that certain classes of alternative choices will be successful in lighting 1 without lighting 2.

The class $v \xrightarrow{1} z$, includes choices $3 \xrightarrow{1} 1$ and $5 \xrightarrow{1} 1$, where the relation of 5 to 2 is such that 2 will not become activated by 5 and hence 9 does not become involved. The class $v,w \xrightarrow{1} z$ consists of $3,5 \xrightarrow{1} 1$, where 9 again does not become involved. The class $v \xrightarrow{1} z$; $v,w \xrightarrow{0} y$ consists of $5 \xrightarrow{1} 1$ and $5,9 \xrightarrow{0} 2$ (i.e., $5 \xrightarrow{1} 2$, and $9 \xrightarrow{-1} 0$). Finally, the class $v,w \xrightarrow{1} z$; $w,x \xrightarrow{0} y$, consists of $3,5 \xrightarrow{1} 1$, and $5,9 \xrightarrow{0} 2$.

Probabilities underlying future choices can be computed similarly, after elimination of impossible alternatives from the universe.

Model 1 has now been carried as far as it needs to be for the experimental data to be reported. Models 2 and 3 will now be discussed.

MODELS 2 AND 3

Since these two models are related they will be taken up jointly. Differences between the models will be pointed out whenever they occur.

1. The subject's goal. As in the case of Model 1, it is assumed that the subject, having been given the experimental task, breaks the total goal of solving the task up into a series of sub-goals, the most immediate of which is to light the center light from the lights directly impinging on it.

2. The universe of alternative choices. In Model 1 this universe was constructed on the assumption that the subject perceives and considers at all times the configuration of $1,2,6 \rightarrow C$ *as a totality* and never considers a possible value of an arrow in this graph apart from the values which the other arrows can have conjointly. In Models 2 and 3 such an assumption is not made. Rather, it is assumed that the subject will assign values to the arrows totally independently from any considerations of the values which other arrows might have, except where the value of a given relationship requires consideration of more than one arrow. Thus, in supposing that 1 might be sufficient to light C ($1 \xrightarrow{1} C$) the subject defines this as an event in his mind and does not bother about the various value conditions for other arrows (i.e., alternatives a, g, h, l, and o in Figure 5 listed for Model 1) under which $1 \xrightarrow{1} C$ could indeed occur. Likewise, in considering that 1 may be necessary to light C but not sufficient, the subject may propose $1,2 \xrightarrow{1} C$, or $1,6 \xrightarrow{1} C$ as possible events, again without bothering about possible values for the relation between the third light and the center. In brief, the universe of alternative choices is different in Models 2 and 3 than it is in Model 1. This universe, at the start of the problem, is indicated in Figure 10.

The probabilities of classes of alternatives related to the lighting of C occurring in a population of subjects may again be listed (see Table 4).

So far Models 2 and 3 are identical. However, they differ in the effect which experience has on the composition of the universe of alternative choices. An example will clarify the differences. Suppose that on trial 1 the subject decided to see whether 1

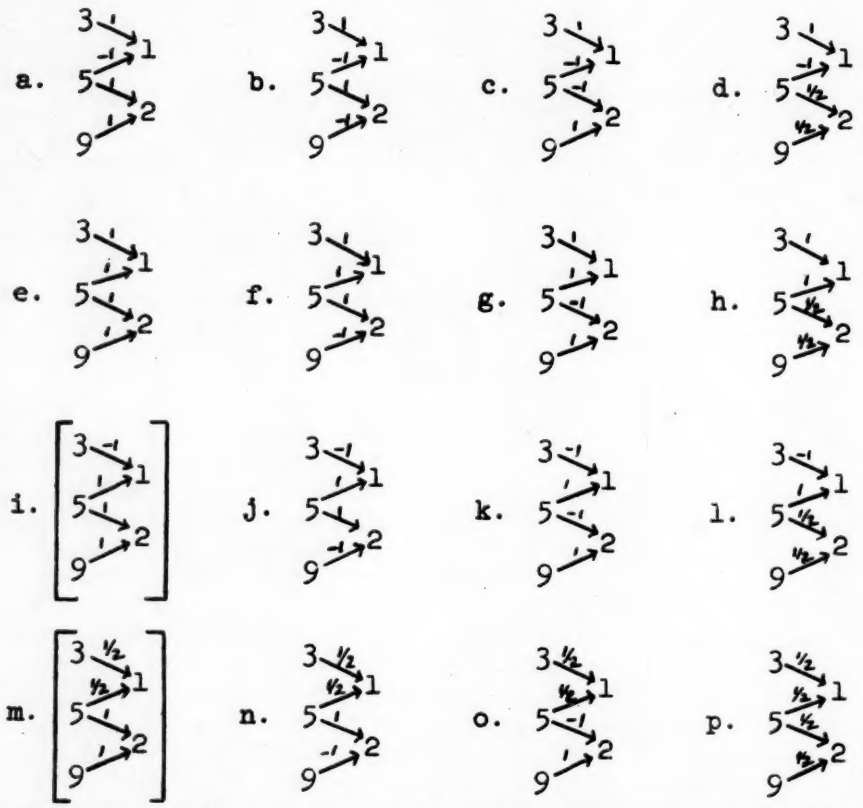

FIG. 9[4]. The universe of alternative choices with regard to the convergence of 3 and 5 on 1, and of 5 and 9 on 2, on the first trial which is made in respect to that convergence.

and 2 together light C. The subject would have found that 1 and 2 together do not light C. The assumption made in Model 2 is now that the only effect which the latter finding has on the universe is the elimination of alternative d (see Figure 10), so that for trial 2 the probability values in Table 4 would be 3/6, 2/6, and 1/6, respectively. Thus in Model 2 the seven events are treated at all times as totally independent. In Model 3, on the contrary, the assumption is made that *experience* can break up the initial independence between alternatives in the universe. Thus, if the finding has been that $1,2 \xrightarrow{1} C$ is false, it would mean in Model 3 not only the elimination of alternative d from the universe but also the elimination of the independence between alternatives a and b (see Figure 10). That is to say, once $1 \rightarrow C$ and $2 \rightarrow C$ have been joined together in experience, they can no longer be considered separately. Thus, the subject will no longer postulate that $1 \xrightarrow{1} C$ *or* that $2 \xrightarrow{1} C$, but rather that $1 \xrightarrow{1} C$,

[4] The bracketed alternatives, while logically possible, are nevertheless eliminated inasmuch as they require that 1 can never be lit without simultaneously lighting 2 and thus preclude a solution to the problem.

TABLE 3

THE PROBABILITY THAT CHOICES OF A GIVEN CLASS WILL BE MADE BY A GROUP OF SUBJECTS IN RESPONSE TO THE CONVERGENCE OF 3 AND 5 ON 1, AND 5 AND 9 ON 2, ON THE FIRST TRIAL IN RESPECT TO THAT CONVERGENCE

Classes of alternative choices	Probability
$V \xrightarrow{1} Z$	12/19 = .63
$V,W \xrightarrow{1} Z$	4/19 = .21
$V \xrightarrow{1} Z$; $V,W \xrightarrow{0} Y$	2/19 = .10
$V,W \xrightarrow{1} Z$; $W,X \xrightarrow{0} Y$	1/19 = .05

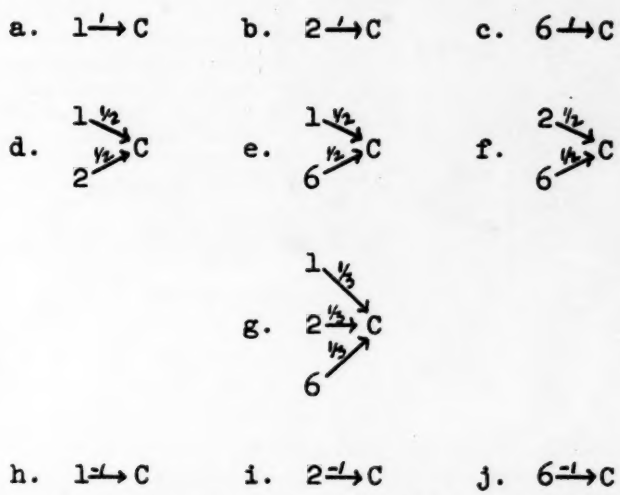

FIG. 10. The universe of alternative choices with regard to the convergence of 1, 2, and 6 on C on the first trial which is made in respect to that convergence.

if and only if $2 \xrightarrow{-1} C$, or that $2 \xrightarrow{1} C$, if and only if $1 \xrightarrow{-1} C$. The way in which this situation may be described conceptually is that the subject fluctuates between two universes which may be represented as in Figures 11a and 11b, respectively.

The relation between Models 2, 3, and 1 is now clear. In Model 2 total independence among alternatives is assumed. In Model 3, dependence among alternatives grows as indicated by experience. In Model 1 the dependence between alternatives is assumed from the outset.

Models 2 and 3 will now be applied to the next major steps in the problem-solving process, namely, those which follow upon the successful lighting of C from the lights which directly impinge on it. The subject will have found out that 1 and 6 together light the center. If the subject has tried one other pair of lights in addition to 1 and 6 he will, assuming Model 3, know in addition that 2 inhibits C.[5] Assuming Model 2, the subject would know that 2 inhibits C if in addition to trying the combination 1,6, he had also tried 1,2; 2,6; 1,2,6; as well as 2 by itself.

Application of Models 2 and 3 to this new choice situation again follows the routine pattern of outlining the goal and the universe.

1. The goal(s). As in the case of Model 1, two assumptions may be made with regard to goals.

a. The subject's goal is to light 1 from the lights directly impinging on it.
b. The subject's goal is to light 1 without also lighting 2.

2. The universe of alternative choices. a. If assumption a is made (i.e., the goal is "to light 1") the subject will be interested in the lights which impinge on 1. The universe of alternative choices is given in Figure 12.

TABLE 4

THE PROBABILITY THAT CHOICES OF A GIVEN CLASS WILL BE MADE BY A GROUP OF SUBJECTS IN RESPONSE TO THE CONVERGENCE OF 1, 2 AND 6 ON C, ON THE FIRST TRIAL IN RESPECT TO THAT CONVERGENCE

Classes of alternative choices	Probability
$W \xrightarrow{1} Z$	3/7 = .43
$W,X \xrightarrow{1} Z$	3/7 = .43
$W,X,Y \xrightarrow{1} Z$	1/7 = .14

[5] Suppose he has tried $2,6 \rightarrow C$ first and found these do not light C, and he has followed this by $1,6 \rightarrow C$. These two trials eliminate (see Figure 10) alternatives f, h, j, g, d and b (since if $2 \rightarrow C$ were equal to ½ or 1, then 2, 6 would have lit C, since $6 \rightarrow C$ has a value of either 1 or ½). This leaves $2 \xrightarrow{-1} C$ as the only possible value for $2 \rightarrow C$.

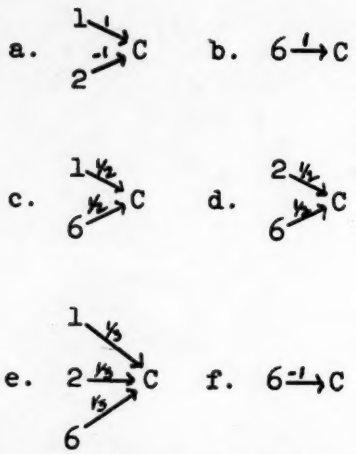

FIG. 11a. The universe of alternative choices, given the assumption that $2 \xrightarrow{-1} C$.

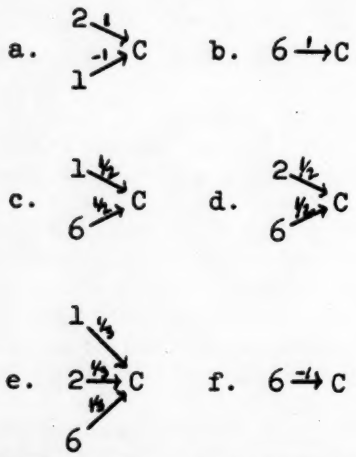

FIG. 11b. The universe of alternative choices, given the assumption that $1 \xrightarrow{-1} C$.

The probability with which the classes of alternative choices $w \xrightarrow{1} z$, and $w,x \xrightarrow{1} z$ will occur in a number of subjects is given in Table 5.

The class $w \xrightarrow{1} z$ is, of course, made up of choices $3 \xrightarrow{1} 1$ or $5 \xrightarrow{1} 1$, and the class $w,x \xrightarrow{1} z$ is made up of the choice $3,5 \xrightarrow{1} 1$. If the subject chooses $3 \xrightarrow{1} 1$, he finds that 3 does not light 1 and he can then proceed to select among the two remaining choices. If he chooses $5 \xrightarrow{1} 1$, he will find that 5 does not light 1 but that 5 lights 2 (which he knows must be avoided). It is assumed again that under these conditions the subject cannot restrict his attention solely to the lighting of 1, and hence that his behavior now falls under assumption b with regard to his choice of goals (i.e., to light 1 without 2).

TABLE 5

THE PROBABILITY THAT CHOICES OF A GIVEN CLASS WILL BE MADE BY A GROUP OF SUBJECTS IN RESPONSE TO THE CONVERGENCE OF 3 AND 5 ON 1, ON THE FIRST TRIAL IN RESPECT TO THAT CONVERGENCE

Classes of alternative choices	Probability
$W \xrightarrow{1} Z$	2/3 = .67
$W,X \xrightarrow{1} Z$	1/3 = .33

The universe of choices and the probability of their selection consequent upon assumption b regarding the subject's goal will now be taken up.

b. If assumption b is made the subject will from the outset be interested in the lights which impinge on 1, those which impinge on 1 and 2 simultaneously, and those which impinge on 2. The subject can now no longer be concerned with $5 \rightarrow 1$ independently but he has to consider possible values of $5 \rightarrow 1$ interdependently with possible values of $5 \rightarrow 2$ and $9 \rightarrow 2$. However, he will still consider $3 \xrightarrow{1} 1$ as an independent event, and it is here that Models 2 and 3 differ from Model 1 even under assumption (b). The universe of possible choices is given in Figure 13.

Alternatives d, e, h, and i can appear without reference to $5 \rightarrow 2$ and $9 \rightarrow 2$ inas-

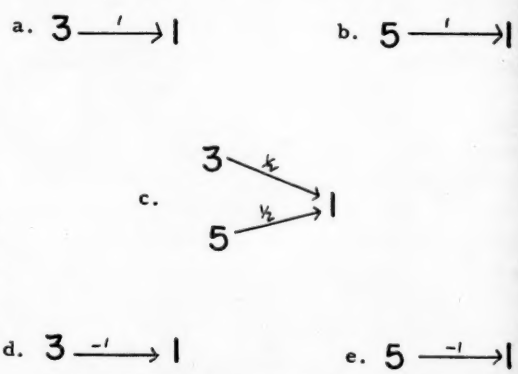

FIG. 12. The universe of alternative choices with regard to the convergence of 3 and 5 on 1, on the first trial which is made in regard to that convergence.

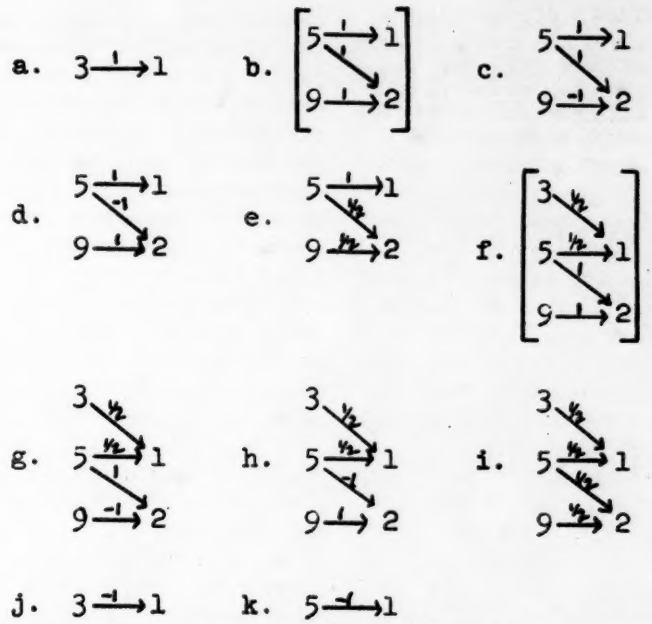

FIG. 13. The universe of alternative choices with regard to the convergence of 3 and 5 on 1, and 5 and 9 on 2, on the first trial in respect to that convergence.

much as they preclude the lighting of 2 simultaneously with 1. The bracketed alternatives are again logically but not practically possible.

In Table 6 are listed the probabilities that certain classes of alternative choices will be successful in lighting 1 without 2.

Probabilities underlying future choices can again be computed after elimination of alternatives which are excluded by virtue of the subject's knowledge of the outcome of previous trials.

The description of Models 2 and 3 is now complete and the three theoretical questions concerning the control process have been dealt with in these models. That is to say, (1) the universe of alternative choices has been outlined, (2) biases in a random scanning process can be identified and quantified using these models as they can in the case of Model 1, and (3) it has been pointed out that in Model 2 the universe is composed of independent events and is fixed, whereas in Model 3 the universe is not fixed but dependence between events is developed as a function of experience.

The differential accuracy attained by the three models in predicting a sequence of choices which subjects make in solving the experimental task will now be discussed. A further test of the model will consist of an effort to demonstrate that a change in the problem solver (i.e., fatigue) can be represented as a quantitative change in the parameters entering into the models. The implications of the latter test are to lend generality to the models developed.

EXPERIMENT 1

In the experiment to be reported first, 77 subjects were given the experimental prob-

TABLE 6

THE PROBABILITY THAT CHOICES OF A GIVEN CLASS WILL BE MADE BY A GROUP OF SUBJECTS IN RESPONSE TO THE CONVERGENCE OF 3 AND 5 ON 1 AND 5 AND 9 ON 2, ON THE FIRST TRIAL IN RESPECT TO THAT CONVERGENCE

Classes of alternative choices	Probability
$V \xrightarrow{1} Z$	3/7 = .43
$V,W \xrightarrow{1} Z$	2/7 = .29
$V \xrightarrow{1} Z;\ V,W \xrightarrow{0} Y$	1/7 = .14
$V,W \xrightarrow{1} Z;\ W,X \xrightarrow{0} Y$	1/7 = .14

TABLE 7

OUTPUT DATA FOR A TYPICAL SUBJECT: THE NUMBER OF TRIALS, THE TIME PER TRIAL (IN SECONDS), AND THE SEQUENCE OF BUTTON PUSHES DURING EACH TIME PERIOD WHICH CONSTITUTES A TRIAL

Trial No.	Time	Trial			
		Time Period 1	Time Period 2	Time Period 3	Time Period 4
1	100	12			
2	08	6			
3	36	16			
4	46	35			
5	64	359			
6	71	68			
7	71	5			
8	167	39			
9	299	4	56	—	6
10	80	8			
11	90	4	6	5	6

lem presented in Figure 2.[6] They were told to work at their own speed and go about solving the problem in any way they wished. Basic "out-put" data which were recorded by the experimenter were the sequence of *trials* used by the subject to solve the problem and the time per trial. A trial is defined as a sequence of button pushes which results in an uninterrupted sequence of lights flashing on the display board of the machine. Thus a subject might push buttons 3 and 5 simultaneously and this would constitute a trial. A typical sequence of trials and times per trial in seconds for the problem presented in Figure 2 is reproduced in Table 7.

It may be noted that three trials are expended until the subject finds out what lights C (i.e., $1,6 \xrightarrow{1} C$). Following this the subject is obviously concerned with the lighting of 1, trying out (3,5) in the process, and finding that $3,5 \xrightarrow{1} 1$ but also that $5 \xrightarrow{1} 2$. Following this he tries to light 1 without lighting 2 by including a 9 in the (3,5) combination, the 9 being intended to inhibit the 2. He finds that the (3,5,9) combination indeed has the intended effect of lighting 1 without 2. He then has to worry about lighting 9 which he does via the (6,8) combination and which turns out to be successful. He now is in possession of all necessary facts to arrive at the solution which would be to press 4, 6, 5, 6 in successive time periods. From a tape recording which was made of this session, it is apparent, however, that at this point (Trial 7) the subject feels the problem is getting too complicated. Thus instead of arriving at a solution, trial 7 is designed to find out whether 5 indeed lights 2. He finds that 5 *does* light 2. Trial 8 now represents the effort to do without 5 and use 3 to light 1. However, the idea of using 5 still seems to linger in his mind because he also pushes 9 which serves no purpose at all in the absence of 5. Trial 8 can be characterised as a decided attempt at simplification of the problem, as an act which disregards some past information (i.e., is regressive), and finally as an act in which the regression itself is somehow illogical [i.e., the subject should have regressed on trial 8 to 3—but not to (3,9)]. In the final trials the subject tries to act within the reality of the situation, except for trial 10 which is redundant.

The subjects in this and the following experiment were not individuals but groups of three individuals who had to reach agreement on trials by unanimous decision. The question may of course be raised whether these groups can be treated legitimately as individual subjects. It may be asked whether anything is altered in the control process of the single individual by the addition of two persons in his environment. A description of the group process may suggest an answer to this question. In each person there was still in evidence an active control process as described in the theoretical models. It is certainly true that from moment to moment, any one member of the group, acting as the spokesman for the group, exemplified all the phases of a control process. It is further evident that one person, possibly the brightest member of the group, would act

[6] Preceding this particular problem the subjects went through a training period in which they were presented with the concepts of sufficiency, necessity, and inhibition, and in which they were given a simple practice problem which however did not have the same "gimmick" present in the experimental problem (i.e., the inhibition of the inhibitor to the center) but was merely designed to demonstrate their understanding of the basic concepts.

more and more as the group's spokesman as time passed. This person again would definitely go through all the phases of a control process, as if an individual working by himself. He would, however, make use of information, analyses, etc., provided by his fellow group members, the total group process suggesting an individual process in which the individual has a larger observation and scanning capacity. The differences between the group's and an individual's performance are therefore assumed to be quantitative rather than qualitative, and expressable as a difference in the quantitative values assigned to the constants in the theoretical models. Using groups of three cooperating subjects who had to reach unanimous decision by means of prior discussion, it was hoped that decisions for trials would be based maximally on a careful consideration of facts and available alternatives.

Each of the three models predicts the probability that a subject will make a class $w \xrightarrow{1} z$, $w,x \xrightarrow{1} z$, $w,x,y \xrightarrow{1} z$, etc., choice on a given trial. Thus for a group of subjects, each model predicts the number of subjects whose choice on any given trial will probably fall into one class or another. The predictions made by the models were checked against obtained experimental findings for the following trials.

1. Trial 1: this is the subjects' first trial on the experimental task, in which they are interested in finding out what combination of lights directly impinging on C will light C.[7]

2. Trial 2a: this is the second attempt to light C for those subjects who on Trial 1 made a class $w \xrightarrow{1} z$ choice. The question investigated here is: what class of choice will the subjects make on their second trial if their first trial was a $x \xrightarrow{1} z$ type choice and if it was not successful in lighting C?

3. Trial 2b is the second attempt to light C for those subjects whose first attempt consisted of a $w,x \xrightarrow{1} z$ type choice which was not successful in lighting C.

4. Trial 3a is the third attempt to light C for those subjects whose first and second attempt consisted of $w \xrightarrow{1} z$ type choices both of which were unsuccessful in lighting C.

5. Trial 3b is the third attempt to light C for subjects whose first and second attempts consisted of a $w \xrightarrow{1} z$ followed by a $w,x \xrightarrow{1} z$ type choice, or vice versa, both of which were unsuccessful in lighting C.

6. Trial 4 (Assumption a) is the trial which follows a successful lighting of C. It is the attempt to light 1 after it has been found that 1,6 lights C and 2 inhibits it.

7. Trial 4 (Assumption b) concerns the attempt to light 1 from the lights impinging on 1 without lighting 2.

8. Trial 5a (Assumption b) is the second attempt to light 1 for subjects whose first attempt consisted of trying $5 \xrightarrow{1} 1$ and who found that 5 does not light 1 but 2 instead which must now be avoided or inhibited.

9. Trial 5b (Assumption a) is the second attempt to light 1 for subjects whose first attempt consisted of trying $3 \xrightarrow{1} 1$ and found that 3 does not light 1.

10. Trial 5b (Assumption b) is the second attempt to light 1 without 2 for subjects whose first attempt consisted of trying $3 \xrightarrow{1} 1$ and were not successful.[8]

The predictions made by each model were checked against the obtained experimental findings by means of a chi-square test of agreement between a priori frequencies and obtained frequencies. The frequencies predicted and obtained are, of course, the frequencies with which subjects' choices, on a given trial, fall into the various classes of choices.[9] That model, or those models, will be rejected as adequate predic-

[7] Of the 77 subjects, 51, or two-thirds of the population, approached the problem trying to find how to light C from the buttons directly impinging on C, and then 'work back.' Twenty-six started out working 'circle buttons' first and then reverted to the above strategy. To retain the conditions worked out for the models, namely that the subject 'works back' from C, the latter group of subjects was not included in the analysis.

[8] This sequence of trials tested does not, of course, exhaust the population of all possible sequences. However, the trials listed above constitute those for which the N was large enough to warrant testing for statistical significance.

[9] In all tests, except those concerning Trial 1, the frequency tables from which chi-squares are computed are 2×1 tables. In order to work with an adequate N, frequencies in the $w \xrightarrow{1} z$ class are compared with frequencies in $w,x \xrightarrow{1} z$ class plus all other pertinent classes.

tions of the choice process for which a significant disagreement between prediction and obtained result can be demonstrated.

Findings

The findings are presented in Table 8. The ten columns represent the ten trials listed earlier which were investigated. The three columns within each of these ten represent, respectively, the differences between the percentages predicted for each class of choice and the percentages obtained; the degree of closeness, expressed by means of chi-square between predicted and ob-

TABLE 8*†

		1			2			3			4			5		
		Trial 1			Trial 2a			Trial 2b			Trial 3a			Trial 3b		
		% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p
1	No Bias	.42	9.70	.01	.52	21.00	<.001	.67		<.001	.78		<.001	.06	.05	.83
	S Bias	.00	.00	1.0	.14	.56	.45	1.06		<.001	.50	4.7	.03	.44	3.5	.05
	P Bias	—	—	—	1.04		<.001	.59	2.93	.08	.94		<.001	.16	.34	.58
	S & P Bias	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0	.40	2.50	.11	.44	3.5	.05
2	No Bias	.28	4.2	.04	.38	4.54	.03	.52		<.001	.66		<.001	.28	1.04	.30
	S Bias	.00	.00	1.0	.10	.27	.60	.82	12.20	<.001	.44	3.59	.06	.08	.08	.78
	P Bias	—	—	—	1.04		<.001	.59	2.93	.08	.94		<.001	.16	.34	.58
	S & P Bias	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0	.38	2.31	.12	.08	.08	.78
3	No Bias	.28	4.2	.04	.38	4.54	.03	.32	1.92	.16	.66		<.001	.48	3.11	.07
	S Bias	.00	.00	1.0	.10	.27	.60	.62	6.67	.01	.44	3.59	.06	.20	.48	.50
	P Bias	—	—	—	1.04		<.001	.59	2.93	<.08	.94		<.001	.16	.34	.58
	S & P Bias	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0	.38	2.31	.12	.20	.48	.50

		6			7			8			9			10		
		Trial 4a			Trial 4b			Trial 5a			Trial 5b					
		Assumption a			Assumption b			Assumption b			Assumption a			Assumption b		
		% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p
1	No Bias	.32	5.8	.02	.40	8.58	<.01	.72	10.72	.001	.18	.54	.48	.52	4.86	.03
	S Bias	.02	.05	.82	.08	.56	.48	.08	.06	.82	.20	.80	.35	.26	1.41	.22
	P Bias	—	—	—	—	—	—	.28	1.58	.20	.82	4.4	.03	.82	4.4	.03
	S & P Bias	.02	.05	.82	.08	.56	.48	.00	.00	1.0	.32	2.38	.12	.38	3.76	.05
2	No Bias	.32	5.8	.02	.80		<.001	.72	10.72	.001	.18	.54	.48	.52	4.86	.03
	S Bias	.06	.03	.88	.70		<.001	.18	.57	.45	.06	.08	.80	.28	1.37	.24
	P Bias	—	—	—	—	—	—	.28	1.58	.20	.82	4.4	.03	.82	4.4	.03
	S & P Bias	.06	.03	.88	.70		<.001	.10	.43	.50	.16	.45	.50	.18	.53	.47
3	No Bias	.32	5.8	.02	.80		<.001	.72	10.72	.001	.18	.54	.48	.52	4.86	.03
	S Bias	.06	.03	.88	.70		<.001	.18	.57	.45	.06	.08	.80	.28	1.37	.24
	P Bias	—	—	—	—	—	—	.28	1.58	.20	.82	4.4	.03	.82	4.4	.03
	S & P Bias	.06	.03	.88	.70		<.001	.10	.43	.50	.16	.45	.50	.18	.53	.47

* The N's for trials 1 through 5b are, respectively, 51, 29, 17, 15, 13, 48, 21, and 17.

† Empty chi-square cells indicate that the chi-square was not computed inasmuch as its p-value could be judged to be extremely significant. A dash in a cell indicates that the particular cell is not relevant.

tained frequencies for classes of choices; and the probability that the given value for chi-square points to a chance difference between theoretical prediction and obtained results. The three main rows in Table 8 denote the three theoretical models tested. Each row is further sub-divided into four parts. In the *No Bias* row are explored the predictions made from a model when a random scanning process is assumed. In the *S-Bias* row the model is tested under the assumption of scanning with a simplicity bias. *P-Bias* denotes a persistence bias and *S & P Bias* denotes simplicity *and* persistence bias combined. These biases are computed as follows.

It was found on Trial 1 that the assumption of random scanning did not lead to very accurate predictions for any of the models. The data showed a consistent subject bias toward the selection of $w \xrightarrow{1} z$ class choices over $w,x \xrightarrow{1} z$ or $w,x,y \xrightarrow{1} z$ class choices, and of $w,x \xrightarrow{1} z$ choices over $w,x,y \xrightarrow{1} z$ choices. The probabilities computed under the assumption of random scanning for each model were now multiplied by constants expressing a simplicity bias (one constant for each class of choice), such that predictions from the model perfectly matched the obtained result. (Thus for Trial 1 the S-Bias row for each model shows a 0% difference, a 0 chi-square and a 1.0 p value.) These constants were assumed to be representative for the population and were applied throughout all further trials. It was found further that on Trial 2a, for example, S-Bias predictions, while fairly accurate, did not take account of a second bias, namely the bias to persist in, and 'run-out,' a certain class of choice once it had been chosen on a previous trial. The S & P bias was computed by calculating the bias toward persistence which together with the bias for simplicity (which latter had been assumed and computed for Trial 1 and all successive trials) would give a perfect prediction for Trial 2. The constants thus obtained were again applied on all further trials, except Trial 4, inasmuch as it was assumed that Trial 4 is like Trial 1, i.e., the first trial in a series. It was found that the S & P bias for Trial 2b was larger than for Trial 2a. The difference is probably due to the fact that in Trial 2a the bias toward simplicity is already in the direction of persisting in the earlier choice class, and hence the second bias need hardly be involved, whereas in the case of Trial 2b the two biases act in opposite ways. Consequently S & P biases were computed for Trials 2a and 2b separately. The P-Bias row expresses the assumption that the choices made on successive trials are merely expressions of a persistence in an earlier chosen class.

The following findings are apparent from Table 8.

1. The assumption that the subject scans randomly among the universe of possible choices, no matter what the universe of choices postulated (i.e., no matter what the theoretical model), is not supported by the data. The predictions in most instances deviate significantly from the obtained results (see the p-values in the No-Bias rows for each model). It should be pointed out, however, that in those instances in which the models make different predictions [i.e., Trials 1, 2a, 2b, 3a, 3b, and 4 (assumption b)], Models 2 and 3 come closer to the obtained results than Model 1 four out of six times.

2. The assumption that the subject has a simplicity bias when he scans among possible choices is supported more strongly by the data, at least for some of the models and for some of the assumptions made regarding the subject's goals.

a. The assumption of an S-bias leads, with one exception, to only chance differences between prediction and obtained results in the case of Model 3, if Assumption a regarding goals is made in all instances where there is a choice between Assumptions a and b, i.e., Trials 4 and 5b. (It may be recalled that under Assumptions a and b the goals are "to light 1," and to "light 1 without 2" respectively. The exception is Trial 2b where the p-value is equal to .01.)[10] When Assumption b is made there are two exceptions. The sum of all chi-squares in the former case is equal to 11.69 ($p = .11$),

[10] Significance is assumed to occur where p is smaller than or equal to .05.

with seven degrees of freedom; in the latter it is in excess of 23.78 ($p < .01$). Thus, it may be concluded that Model 2 (S-bias, Assumption a) leads to predictions of successive choices among classes of choices which deviate from the obtained results only in a chance fashion. Assumption b regarding the subject's goals is not supported by the findings.

b. The sum of the chi-squares for Model 2 (S-bias, Assumption a) is equal to 16.82 ($p = .02$). The sum of the chi-squares for Model 2 (S-bias, Assumption b) is in excess of 28.94 ($p < .001$). It may be concluded that Model 2, given an assumption of a simplicity bias and Assumptions a or b regarding the subject's goals, leads to predictions which tend to deviate systematically from the obtained results. As in the case of Model 3, making Assumption a leads to better results than does making Assumption b.

c. It may be concluded by inspection of Table 8 that any assumption regarding type of goal within Model 1 leads to systematic differences between prediction and obtained results under the assumption of a simplicity bias.

3. The assumption that the subject has a simplicity *and* a persistence bias leads to rather accurate predictions within some models and some assumptions.

The assumption of an S & P bias makes for the absence of any systematic difference between prediction and obtained results in the case of Models 2 and 3, except when Assumption b is made regarding the subjects' goal. The predictions made from Model 1 are also much better under these conditions. The sum of the chi-squares for Model 3 (S & P bias, Assumption a), and for Models 2, and 1, are, respectively 3.70 ($p = .60$), 3.30 ($p = .65$), and 8.53 ($p = .15$), using five degrees of freedom.

4. Predictions based purely on the notion of a P-bias do not lead to very accurate prediction.

5. The only instance in which the simplicity adjustment does not improve predictions over the No-bias assumption for Models 2 and 3 is on Trial 2b. This is found to be true also, though to a lesser extent, in the case of the second experiment (Table 10). It may be remembered that Trial 2b presents an instance where the simplicity bias and persistence bias work in opposite directions. Nevertheless, there should be some evidence of a simplicity bias. One possible reason for this finding could be that a (w,x) type combination of buttons lights the center light in the practice problem which is given to the subjects prior to the experimental task and that this past experience adds an additional bias to try further (w,x) type choices in subjects who have already made one such choice.

In summary, it may be said firstly that the most successful predictions can be made when it is assumed that the subject scans a universe of choices with a simplicity bias or a simplicity bias combined with a bias which takes the form of persisting to have a new response fit the same choice class as an earlier response. (This bias could also be called a bias toward similarity.) Success in prediction is not attained when the assumption of random scanning is made. Secondly, the data lead one to suspect that the universe within which the subject scans for possible choices is not one that is fully elaborated at the outset (Model 1), nor one which is overly simplified and remains so (Model 2). Rather, it is a universe in which the various alternatives are largely considered independently from one another at the outset but in which these alternatives become joined into units and lose their independence when required by experience (Model 3). Thirdly, and related perhaps to the second main finding, is the finding that the subject has a tendency to start with simple goals enlarging and complicating these when required by experience.

EXPERIMENT 2

Thus far models have been presented for describing the thinking process (control process) which may be invoked to account for the observed behavior with reference to the present experimental task. These models postulate the particular universe of choices which a subject thinks are available to him at a given moment; they postulate various modes of scanning among alternatives that

could conceivably be employed by a control system; and, finally, these models postulate various modes of change in the universe as a result of experience. The models outlined are general and can presumably be applied to any task (as long as it can be expressed in the calculus of propositions), and to any person. The second experiment explores the generality of the models with respect to the latter question. That is to say, if there is a general model for the thinking process, the difference in the thinking of specific individuals can be understood and predicted by mere quantitative changes in the parameters of the model. The differences in the thinking of specific individuals which will be explored here are the differences in the thinking of individuals who are fatigued and those who are not. Specifically, the models discussed make choice behavior a function of three parameters: (1) the probability of a choice being chosen out of a universe of choices, (2) the simplicity of that choice, and (3) the similarity of that choice to a just prior choice. Given a general model, fatigued subjects should differ from normal subjects in terms of the quantitative values which need to be assigned to the three parameters listed above.

Before presenting the findings a brief description will be given of the fatigue experiment. The subjects in this experiment (24 groups in all) were given the same task as the subjects in the first (non-fatigue) experiments. However, the fatigued subjects started to work on the experimental task at 1 a.m. after having participated from 7 p.m. to 1 a.m. in a reaction time experiment involving them uninterruptedly (except for two five-minute breaks) in an activity requiring rapid muscular reactions (flipping switches in concert with one fellow group member) to incoming visual stimuli.

Findings

Quantitative differences between fatigue and control subjects in the parameters of simplicity and persistence will be presented first. Following this, evidence concerning the success of each model in predicting the obtained results will be presented, indicating a shift, or absence thereof, in the universe which generates the probability value for each choice. In Table 9 are presented the percentage of subjects who made a $w \xrightarrow{1} z$ class choice (i.e., the simplest choice) on each of the trials which have been investigated earlier, except for Trial 3b which did not occur among the fatigue groups in adequate number. It might be expected that under fatigue conditions subjects will show an increased bias toward a $w \xrightarrow{1} z$ type choice, i.e., toward simplicity and/or persistence.

TABLE 9

PERCENTAGE OF SUBJECTS MAKING $W \xrightarrow{1} Z$ TYPE CHOICES ON A SEQUENCE OF TRIALS UNDER FATIGUE AND NON-FATIGUE CONDITIONS

	Percentage making a $W \xrightarrow{1} Z$ type choice						
	Trial 1	Trial 2a	Trial 2b	Trial 3a	Trial 4	Trial 5a	Trial 5b
Non-Fatigue	.57	.52	.24	.53	.83	.59	.86
Fatigue	.59	.87	.33	.78	1.00	.71	.92

Without any exception a higher percentage of fatigued subjects make $w \xrightarrow{1} z$ type choices. Using a sign-test and making use of the fact that the difference is in the predicted direction (i.e., using a one-tail test), the obtained difference is significant at the .025 level. The differences in Table 9 are generally substantial except in the case of Trial 1 (i.e., *early* in the problem-solving venture), Trial 2b (i.e., where the simplicity and persistence biases work in opposite directions), and Trial 5b (perhaps because both percentages are high to begin with). The simplicity bias and persistence bias work in the same direction in most of the trials listed in Table 9. Thus it cannot be concluded whether one or both biases are higher for fatigued subjects. However, working out the predictive success of each model and computing the two biases (see below), it turns out that both are higher for fatigued subjects.

The success of the various models in predicting successive choice behavior, giving an indication of the sampling universes employed as well as another check on the

TABLE 10*

		Trials											
		Trial 1			Trial 2a			Trials 2b			Trial 3a		
		% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p
1	No Bias	.46	3.9	.05	1.23	3.81	.05	.50	.37	.54	1.28	3.75	.05
	S Bias	.00	.00	1.0	.79	1.84	.17	.90	1.33	.25	.96	2.16	.14
	P Bias	—	—	—	.26	.53	.46	.66	1.2	.27	.44	1.25	.26
	S & P Bias	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0	.02	.01	.90
2	No Bias	.32	1.75	.20	1.09	2.93	.09	.34	.17	.68	1.16	3.01	.08
	S Bias	.00	.00	1.0	.77	1.68	.20	.64	.66	.42	.92	1.98	.16
	P Bias	—	—	—	.26	.53	.46	.66	1.2	.27	.44	1.25	.26
	S & P Bias	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0
3	No Bias	.32	1.75	.20	1.09	2.93	.09	.14	.02	.88	1.16	3.01	.08
	S Bias	.00	.00	1.0	.77	1.68	.20	.46	.33	.58	.92	1.98	.16
	P Bias	—	—	—	.26	.53	.46	.66	1.2	.27	.44	1.25	.26
	S & P Bias	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0	.00	.00	1.0

		Trials													
		Trial 4						Trial 5a			Trial 5b				
		Assumption (1)			Assumption (2)			Assumption (2)			Assumption (1)			Assumption (2)	
		% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p	% diff.	x^2	p
1	No Bias	.66	3.60	.06	.74	10.67	<.001	.84	8.34	<.01	.42	.30	.59	.76	1.01	.30
	S Bias	.32	1.64	.20	.36	1.76	.19	.16	.52	.49	.08	.03	.85	.08	.03	.85
	P Bias	—	—	—	—	—	—	.16	.52	.49	.58	1.20	.26	.58	1.20	.26
	S & P Bias	.32	1.64	.20	.36	1.76	.19	.16	.52	.49	.46	.64	.43	.46	.64	.43
2	No Bias	.66	3.60	.06	1.14		<.001	.84	8.34	<.01	.42	.30	.59	.76	1.01	.30
	S Bias	.40	2.00	.15	.82		<.001	.16	.52	.49	.26	.16	.70	.10	.04	.54
	P Bias	—	—	—	—	—	—	.16	.52	.49	.58	1.20	.26	.58	1.20	.26
	S & P Bias	.40	2.00	.15	.82		<.001	.16	.52	.49	.40	.47	.50	.44	.56	.45
3	No Bias	.66	3.60	.06	1.14		<.001	.84	8.34	<.01	.40	.30	.59	.76	1.01	.30
	S Bias	.40	2.00	.15	.82		<.001	.16	.52	.49	.26	.16	.70	.10	.04	.54
	P Bias	—	—	—	—	—	—	.16	.52	.49	.58	1.20	.26	.58	1.20	.26
	S & P Bias	.40	2.00	.15	.82		<.001	.16	.52	.49	.40	.47	.70	.44	.56	.45

* The N's for trials 1 through 5b are respectively, 17, 9, 6, 9, 18, 12, and 7.

models, is presented in Table 10 which is arranged similarly to Table 8.[11]

1. The assumption that the subject scans randomly among the universe of possible choices is again not supported by the findings for any model. It may again be pointed out, however, that in those instances in which the models make different predictions, Models 2 and 3 come closer to the obtained results than Model 1 in four out of five cases.

2. The assumption of a simplicity bias leads to better predictions. The sum of the chi-squares and the p values under Assumption a with regard to goals (where there is a choice between a and b) for Models 3, 2, and 1 are, respectively, 6.67 (p = .45), 7.00 (p = .41), and 7.52 (p = .37). Assumption

[11] Because of the smaller N in the fatigue experiment, Yate's correction has been used in computing chi-square whenever the expected cell frequency is less than 5.

b with respect to goal again tends to lead to less accurate predictions than Assumption a. The order of success in prediction for the three models is the same as that found in the first experiment. Fewer significant p-values are found in the fatigue experiment as a result of a smaller N.

3. Under the assumption of a simplicity and persistence bias, and Assumption a with respect to the subject's goal (where there is a choice), the chi-squares for Models 3, 2, and 1 are, respectively, 2.99 (p = .70), 2.99 (p = .70), and 2.81 (p = .72), or essentially identical.

4. Predictions based purely on the notion of a persistence-bias are again less accurate.

In summary, it can be stated firstly, that it seems possible to express the difference in the thinking process between fatigued and normal subjects by a quantitative difference in the same parameters of the theoretical models (i.e., quantitative differences in the simplicity, or persistence bias, or both). Thus, thinking models of some generality seem to have been constructed. Secondly, the most successful predictions can be made when it is assumed that the subject scans a universe of choices with a simplicity bias or a combined simplicity and persistence bias. Success in prediction is not obtained when the assumption of random scanning is made. Thirdly, the data again lead one to suspect that the universe within which the subject scans is not fully elaborated from the outset, nor is it made up of elementary and totally independent alternatives throughout the problem solving process. Rather, the universe is one in which once elementary and independent alternatives become joined into units and lose their independence when required by experience. Fourthly, the subject tends to start with simple goals, enlarging these when required by experience.

DISCUSSION

Three models concerning the nature of a control system within a human subject have been formulated and tested. The nature of the universe of alternative choices constructed by the subject, the scanning among these, and the effect of experience on the universe have been explored, for normal and fatigue conditions. In the present research attention was restricted to a limited sequence of trials, occurring in the initial stages of the problem solving process (approximately the first three to four minutes of a task which on the average took twenty minutes to solve). The models which have been described are rather too simple to predict the *entire* sequence of trials for a given problem. In reality the problem-solving process on the experimental task cannot be understood without consideration of, and constructing a model for, the memory process. The segment of the total graph showing the interrelations between all the lights which is of interest to the subject at a given moment, and therefore the universe of alternative events which a subject considers at any given time, is a function of his accumulated knowledge (i.e., his 'knowledge tree') up to that time. Since the subject is not allowed to record past knowledge, it is a function of his memory. An example may help to clarify what is meant. The only conceivable reason why a subject would explore at a given trial whether 9 inhibits 2, (see Trial 5, Table 7) is because he knows (i.e., remembers) at that time that 5 is necessary to light 1 which in turn is necessary to light C, and that 5 also lights 2 which inhibits C. Therefore, 2 must be inhibited. If it were found that 9 inhibits 2, 9 could be activated at the same time that 5 is activated, the two together producing a 1 but not a 2. What has been said above concerning the past knowledge or memory which the subject brings to a given trial can be represented graphically by the 'knowledge tree' in Figure 14.

If it can be assumed on Trial 5 that Figure 14 represents the subject's 'knowledge tree,' a specification of the universe of choices open to the subject becomes possible. If the 'knowledge tree' is not as

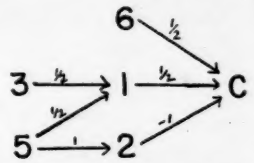

FIG. 14. A subject's 'knowledge tree' at the start of trial 5.

represented in Figure 14 a different universe of alternative choices would have to be constructed. For example, suppose that in the case discussed above the subject did not remember that 2 inhibits C. In that case he would not be concerned on trial 5 with inhibiting 2 and with the universe of possible choices which could lead to this goal, but rather he would see that 2 can be ignored and that there are no further facts to be accumulated (since 4 lights 3) and so, on trial 5, he might try the solution 4, 0, 5, 6 (4 to light 3 in two steps, 5 with 3 lights 1, and 6 with 1 to light C) which is indeed a frequent member of the family of solutions tried. It is clear then that in addition to the models which outline the universe of choices and selection among these, there is need for a model of the memory process itself which will be helpful in predicting what the 'knowledge tree' is likely to be at a given moment.

A 'knowledge tree' existing in the subject's mind at any moment may be looked at as a set of responses made by him over time to a set of stimulus situations. A specific stimulus and a response to it are shown in Figure 15a and 15b respectively. Every time the subject makes a new response he also tends at the same time to respond again, explicitly or by implication, to an earlier stimulus. Thus, when the subject, in the example given earlier, responds to the stimulus $9 \rightarrow 2$ he *uses* his knowledge of, i.e., responds again to, all the prior stimuli $3,5 \rightarrow 1$, $5 \rightarrow 2$, and $1,2,6 \rightarrow C$.

At any given moment the branches of the 'knowledge tree,' being past responses, may differ in terms of the recency and the frequency of the response. In general a 'knowledge tree' will not constitute an undifferentiated whole with respect to these dimensions. In the psychological literature (Overall and Brown, 1957), the frequency and recency of a response or set of responses have been linked to the probability that

FIG. 15. Example of a stimulus and a response typical of the problem solving process.

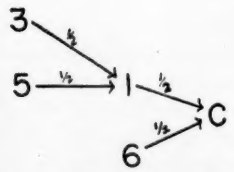

FIG. 16. Alternate 'knowledge tree' for a subject at the start of trial 5.

the same response will be made again. Overall and Brown propose a model in which response probability can be computed from an initial trace magnitude for the response (itself a function of reward expectancy, e.g., probability that the response will lead to success), and a decay coefficient for the trace. Such a model might well be applied to compute the probability that all or parts of an available 'knowledge tree' will form the subject's response to the situation at a given trial. For example, consider the 'knowledge tree' presented in Figure 14. What is the probability that the subject on a given trial will respond to his past experience as in Figure 14 or that he will 'forget' that 2 inhibits C, therefore ignoring the connection between 5 and 2, i.e., in essence basing his actions on a 'knowledge tree' as in Figure 16? Overall and Brown's computations of the response probabilities of these alternate responses to past experience would be as follows. The trial under discussion is the beginning of Trial 5. Suppose that the response that $1,6 \xrightarrow{1} C$ was rewarded on Trial 1 creating an initial memory trace of, say, 100. Suppose further that each successive trial was based, at least by implication, on this rewarded response. Suppose for simplicity that each time the response $1,6 \xrightarrow{1} C$ is made a new initial trace value of 100 is established. Finally, suppose that the decay coefficient is 10% per trial. The memory trace value for $1,6 \xrightarrow{1} C$ after four completed trials then equals 344. Now suppose that the response $2 \xrightarrow{1} C$ was rewarded on Trial 2, had an initial trace value of 100, and was not made use of either explicitly or by implication since then. The memory trace value of $2 \xrightarrow{1} C$ would then be 81 after four completed trials if the same decay is assumed. Finally, suppose the memory trace value for $3, 5 \xrightarrow{1} 1$ and $5 \xrightarrow{1} 2$ equal to 100, after four completed

trials. The probability that the 'knowledge tree' presented in Figure 14 will be the subject's response to his experience on Trial 5 is now simply

$$\frac{344 + 81 + 100 + 100}{344 + 81 + 100 + 100} = 1.0$$

(i.e., p is the sum of the memory traces in the response $\Sigma(mt)r$, divided by all available and relevant memory traces Σmt). The probability that the subject's response will be as indicated in Figure 16 will be

$$\frac{344 + 100}{344 + 100 + 81} = .85.$$

On this basis it is obvious that the former response will be made rather than the latter. However, coupled again with a bias toward simplicity which again could be entered as a constant multiplier, the situation may well arise and does in fact arise where the latter response is made rather than the former.

Application of this model for predicting the likelihood that a subject will use one knowledge tree' or another still hinges on the ability to determine how *time* (needed for the decay function) relates to, i.e., is determined by, the processes inside the control system. It would seem fruitful in this respect to investigate the notion that time spent in the decision process, i.e., time spent on a given trial, is a function of the number of choices in the universe at the time of that trial, and their simplicity, both in terms of the absolute level of simplicity and their similarity in this respect.

In principle an attempt to describe formally the thinking process with respect to the 'PSI' seems hopeful. This, of course, raises the possibility of programming a computer to solve 'PSI' problems as humans seem to do and then operate the computer under a variety of parametric values for the system until certain striking behaviors occur. The suggestion to write computer programs for human thinking processes, of course, is not new and has been made, for example, by Kochen and Galanter (1958). Of particular interest in respect to the present research would be to vary the level of simplicity of choices available to the person, and the biases toward simplicity and persistence and observe the effect on behavior, contrasting particularly behavior marked by the neat step-wise closing in on the solution without redundancy and behavior marked by many regressions, 'irrational' choices, and ultimately complete breakdown.

REFERENCES

John, E. R. Contributions to the study of the problem-solving process. *Psychol. Monogr.*, 1957, 71, 1–39.

John, E. R. & Miller, J. G. The acquisition and application of information in the problem-solving process: An electronically operated logical test. *Behav. Sci.*, 1957, 2, 291–301.

Kochen, M. & Galanter, E. H. The acquisition and utilization of information in problem-solving and thinking. *Information & Control*, 1958, 1, 280.

MacKay, D. M. Towards an information-flow model of human behavior. *Brit. J. Psychol.*, 1956, 47, 30–43.

McCulloch, W. S. & Pitts, W. A logical calculus of the ideas immanent in nervous activity. *Bull. Math. Biophys.*, 1943, 5, 115–133.

Newell, A., Shaw, J. C. & Simon, H. A. Elements of a theory of human problem solving. *Psychol. Rev.*, 1958, 65, 151–167.

Overall, J. E. & Brown, W. L. Recency, frequency, and probability in response. *Psychol. Rev.*, 1957, 64, 314–324.

Rapoport, A. Quantification of performance in a logical task with uncertainty. In *Symposium on information theory in biology*, New York: Pergamon Press, 1958.

Shannon, C. E. A symbolic analysis of relay and switching circuits. *Trans. Amer. Inst. Elect. Eng.*, 1938, 57, 713–723.

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CRITIQUE AND COMMENT

Psychotherapy is a focal point of the on-going debate between the operationally-empirically minded investigator and the seeker of intuitive understanding. The former often dismisses the insights of the latter as insufficiently validated or even as incapable of unambiguous validation. The latter, if he is a clinician, may fail to see how results statistically validated "at the .05 level of confidence" can help him deal with unique and complex troubled persons, whom he is trying to help. The issues of the debate and the knotty dilemmas inherent in the subject-object dichotomy, as it relates to the psychotherapeutic situation, are discussed in this commentary.

SOME COMMENTS ON THE FUTURE OF RESEARCH IN PSYCHOTHERAPY[1]

by Hans H. Strupp

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As you know, we have never prided ourselves on the completeness and finality of our knowledge and capacity. We are just as ready now as we were earlier to admit the imperfections of our understanding, to learn new things and to alter our methods in any way that can improve them.

Sigmund Freud (1919)
(1955, Volume XVII,
p. 159)

THERE is a growing consensus today which says that the future of psychotherapy is that of a scientific discipline. In recent years this point of view has been advanced by numerous writers, who add that any other course would tend to relegate psychotherapy to a doctrine, a faith, or a gospel (Kardiner, 1958; Kubie, 1956). While Freud often spoke of psychoanalysis as a "movement" and "cause," he never conceived of it as anything but a science. Moreover, he feared greatly that institutionalization would lead to stagnation and the emergence of a cult rather than to scientific development. This apprehension, it must be conceded, has not been altogether groundless, but fortunately there is a countertrend; and it is this countertrend with which many of the foremost thinkers in the area ally themselves. I am referring to the increasing emphasis on controlled research and experimentation, that is, the application of the scientific method to furthering our understanding of the psychotherapeutic process. During the past fifteen years the quantity as well as the quality of research contributions has grown, and there is every reason to believe that the coming decades will see an intensification of this effort. Has research exerted a demonstrable influence on the theory and practice of psychotherapy? In my opinion, the influence has been slight, and I propose to examine, in this paper, some of the reasons for this seemingly pessimistic conclusion. I am also prompted by the belief that, somehow we should be able to do better.

First it will be necessary to clarify what 'research contribution' is intended to mean in this context. The term "research" is broad indeed and connotes very different things to different people. To American psychologists and social scientists schooled

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in the tradition of empiricism, positivism, and operationism, the meaning is fairly clear: basically it means the formulation and testing of a hypothesis to aid in building a theory. The ground rules for this process are often intricate and complicated, but the insistence is always on empirical evidence for any assertion and an explicit statement of the means by which knowledge is derived. Such knowledge is public rather than private; it is demonstrable, reproducible, and communicable. These stipulations are those of any science, and they must be adhered to insofar as psychotherapy aspires to the status of a scientific discipline.

To Freud and those working in the psychoanalytic tradition, "research" had a similar but not identical meaning. To be sure, he diligently searched for empirical evidence; he was concerned with making explicit the procedures by which knowledge was acquired; he recognized the need to replicate findings; and he strove for a distinction between clinical findings and the theoretical formulations he devised to explain them. Nevertheless, he was far less rigorous in his investigations than contemporary researchers, if—let it be noted—infinitely more productive. Certainly, one of Freud's greatest and probably most lasting achievements is that of having devised a novel and unique method of investigation (the psychoanalytic situation) and having incisively explored its essential ingredients. Psychoanalytic research was and largely continues to be done in the clinical setting; this setting led to the emergence of the cardinal principles upon which psychoanalysis as a therapeutic technique is built; this setting, too, by its very nature imposes serious restrictions on the rigor which is part and parcel of the scientific method. True, it has its built-in "controls": it defines the therapeutic situation as well as the role of the analyst (within rather broad limits); it seeks to maximize the patient's contribution to the situation while holding the analyst's constant; it imposes relatively strict rules upon the role behavior of the analyst; but it fails to insure that the analyst's observations (and the inferences he draws therefrom) are *reliable*, that is, more than purely subjective experience.[2]

Limiting the discussion to the American scene, it may be noted that knowledge about the psychotherapeutic process is enriched from two major sources: (a) analysts working in the tradition of Freud, and (b) research workers whose approach is more eclectic. The former group, largely on account of the restrictions imposed on psychoanalytic training, consists principally of analysts whose background training has been in medicine; the second group, primarily of psychologists whose training and experience has been in clinical psychology. With the rise of clinical psychology as a science and profession during the last decade, and as a result of increased contact of psychologists with psychoanalytic principles and techniques, research workers bring to bear upon their investigations a vastly greater understanding and appreciation of the subject matter, problems, and concerns of psychoanalysis than was true before 1940.[3] Psychoanalysts, on the other hand, have begun in some instances to seek the collaboration of psychologists whose training has stressed research design and methodology as well as clinical experience in psychotherapy. In short, there seems to be a tendency for psychoanalysts to know more about research (in a rigorous sense) and for research workers to know more about the dynamics of psychotherapy as an interpersonal process. Consequently, there is an expanding meeting ground, hence

[2] Not long ago a British follower of Melanie Klein reported that she was "shocked" to learn from a conversation with an American analyst that he and most practicing analysts in this country do very little research. As she saw it, *every* case at her institute is a "research" case. A similar comment was made to the writer by a British child analyst trained by Anna Freud. There are indications that the younger generation of American analysts, perhaps as a result of more extensive training in the natural and social sciences, tend to use the term "research" in a stricter sense.

[3] A comprehensive survey prepared in 1943 (Sears) revealed a widespread preoccupation to "validate" psychoanalytic concepts and principles through methods then current in academic psychology. Significantly, Sears dispensed with reviewing "A few studies. . .of the psychoanalytic process itself, . . .since none of these was designed to test any theoretical point" (Preface, p. x).

greater possibility for communication and collaboration.

This statement is not intended to suggest that the millenium has arrived. The cleavage between the two kinds of research contributions—with a few notable exceptions—continues to be a rather sharp one. On the one hand, there is a plethora of papers which, while usually starting with clinical observations, pile inference upon inference and arrive at conclusions which typically are not capable of verification. On the other hand, there are numerous investigations whose attention to matters of methodology, sampling, quantitative assessment, and statistical control is reasonably adequate, but which fall short of illuminating the phenomena they purport to clarify. It is noteworthy that by and large the first kind of contribution tends to appear in "psychoanalytic" journals, the second kind in "psychological" ones. It is a hopeful sign that in papers of the first kind there is increasing emphasis on scientific rigor; with respect to researches of the second kind there is greater stress on meaningfulness and relevance. Very probably, the future will bring even greater rapprochement, and perhaps an eventual amalgamation of the two approaches. At present, however, it remains true that the former studies focus on the individual case, stressing longitudinal development, idiosyncratic richness, and depth; the latter place greater confidence in the cross-sectional approach, in which one or a few variables are systematically investigated in a sample of patients, therapists, etc. Here the individuality of the single case is subordinated to considerations of experimental design and statistical inference.

This problem is familiar to all who have witnessed the battle over psychometric *vs.* projective tests, and it seems hardly necessary to belabor the issue. Although new statistical techniques (which are still not very widely employed) have opened new vistas for the study of the individual case, the problem remains with us. Let it be conceded without further discussion that both sides have a contribution to make, and that a scientific discipline of psychotherapy can and does profit both from exhaustive studies of individual cases and from cross-sectional researches. The one may suggest hypotheses which can be more conclusively investigated by the other, and in general there is little danger that either approach will soon have to be abandoned for lack of problems.

At the same time, it is clear that the development of psychotherapy tends in the direction of greater specificity, explicitness, precision. This movement is inescapable if psychotherapy is to be taught, described, and explicated. It is becoming an axiom that concepts must be defined more stringently, and that the vagueness inherent in the subject matter must be diminished. The demands for operational definitions—or at least demonstrable empirical referents—can hardly be sidestepped. These desiderata are expounded with considerable vigor at scientific meetings, but, with the exception of a small minority of researchers, most practicing clinicians and psychotherapists are none too mindful of these injunctions. And, in a sense, rightly so; they cannot suspend their clinical work until such time as research sharpens their tools; they must go ahead, meet the vast clinical demands, and do the best under existing conditions.

The increasing split between the clinical function and the research function is undoubtedly due to many complex reasons, of which the technical difficulty of combining scientific rigor with therapeutic objectives is merely one. By contrast, it is interesting to note Freud's assertion that, in psychoanalysis, therapy and research always go hand in hand. Indeed, he considered this one of the distinctive features of the method. To him, each individual patient was the proving ground for his previous theories and the *fons et origo* of new clinical discoveries, which were then woven into subsequent formulations. He found it impossible to divorce research from the process of therapy, and conceived of himself more as a scientist than as a "healer." In fact, he came to the final conclusion that the possibilities of psychoanalysis as a research tool far overshadowed

those of a treatment technique. To Freud, "research" meant *clinical* research, i.e., the painstaking study of individual patients. However, he applied the scientific method and, through the study of new cases, tested his hypotheses in an approximate way, without however achieving strict experimental control. In some instances, his observations led Freud astray, but it is a tribute to his genius that he succeeded in making valid generalizations on the basis of exceedingly small samples. At any rate, he found the roles of scientist and therapist quite congenial, and the thought of applying a therapeutic technique routinely was utterly alien to him. Nevertheless, as he envisaged the future of psychoanalysis, he foresaw a growing group of analysts who were primarily practitioners rather than researchers and who would rely on others to forge and refine the tools needed for clinical work.[4]

Furthermore, Freud held the view that research on psychoanalysis (as a therapeutic instrument) could *only* be conducted in the psychotherapeutic situation—at least he did not foresee other possibilities. Even today a strong belief is held in many quarters that in order to do research on the psychotherapeutic process, particularly long-term, analytically-oriented psychotherapy, it is mandatory to leave the therapeutic situation undisturbed, even though various research techniques may be used as long as they do not interfere with the therapeutic situation proper. Another group of researchers seems to feel that relevant and meaningful research can be conducted experimentally or quasi-experimentally. The first group, harking back to Freud, asserts that the analytic situation represents a specific method of investigation, and that *psychoanalytic* research cannot be performed without this method; the latter group, without questioning the value of the method for therapeutic and other purposes, believes that other approaches, too, may yield fruitful and provocative data, which eventually, may find their way back into the treatment situation.

So far, I have tried to indicate that essentially there are two kinds of research approaches to the psychotherapeutic process: the clinical, used preponderantly by practicing analysts and therapists; and the experimental, exemplified chiefly by clinical research psychologists. While changes are gradually taking place, it appears that clinical penetration and scientific rigor have varied inversely. Furthermore, the two approaches undoubtedly reflect the temperaments of the proponents. If the advances of psychoanalysis as a therapeutic technique are compared with the experimental research contributions, there can be little argument as to which has more profoundly enriched theory and practice of psychotherapy. To make the point more boldly, I believe that, up to the present, research contributions have had exceedingly little influence on the practical procedures of psychotherapy. This, in view of the advocacy of more and more research, the expansion of facilities and the greater availability of federal and private financial support, is a deeply disquieting state of affairs requiring closer scrutiny. Why is this so? Why have research contributions had so little impact? Is it that they have nothing to teach the practicing therapist? Are therapists so impervious to scientific findings that they are unable to profit from them? Is our knowledge of the subject matter so rudimentary that at this stage research cannot possibly be expected to have left its mark on clinical practices? I raise these questions, not as a therapeutic nihilist but as a researcher who feels that important answers should come from research. But, I must confess that I am not fully convinced that they will. A closer look at the contemporary scene may not enable one to discern trends to forecast the future, but it may help to clarify the schism that divides practicing therapists and investigators doing research on the therapeutic process.

The therapist-clinician might speak as

[4] Rogers (1955), on the other hand, feels that he cannot simultaneously function in both roles: if, in the clinical situation he attempts to be a scientific observer, he feels his empathic ability suffers. He considers the two roles diametrically opposed and essentially irreconcilable.

follows: Psychotherapy, first and foremost, is a clinical art, not a science. My task is to aid and promote personality growth, or, more accurately, to help a troubled human being in his struggle with his emotional difficulties. I assume that the patient is fully capable of steering his own life course once the obstacles which stand in his way are removed. My task is to aid him in this process. I don't "handle" a case, I don't "manipulate" a person, I don't apply a "technique." My whole effort is concentrated upon *understanding* this person and his difficulties in living. If I am a psychoanalyst or an analytically-oriented therapist, I follow certain technical rules (based on such concepts as transference, resistance, defense, etc.) which usually accelerate the therapeutic process. From this vantage point, I don't see how it would help me to know that persons having certain psychological characteristics are better candidates for intensive psychotherapy than persons having some other characteristics; that interpretations of a certain kind tend to produce resistance in certain kinds of patients at a certain stage in therapy; that under certain conditions the therapist's personal problems tend to interfere with the therapeutic process; that certain patterns of personality characteristics in a therapist are more conducive to progress in therapy than certain others; that interpretive techniques are more effective in the hands of certain therapists with certain kinds of patients under certain circumstances, etc. How do such findings help me to understand my patient, Joe Smith, whom I am seeing three times a week, who relates to me, a particular therapist, in a particular way, who tells me about his anxieties in highly specific situations and whose style of life, defensive patterns, life goals, fantasies, wishes are a unique constellation? True, there are important clinical papers which have influenced me deeply: they are the papers of a host of sensitive therapists—not the kinds of investigations which are based on a sample of patients often treated by student therapists, which report results at the .05 or .01 level of statistical confidence. I am in sympathy with the point of view which says that we must achieve greater clarity about our concepts and our operations, but I doubt whether an experimenter standing *outside* the therapeutic situation can deepen my understanding of patients qua living human beings.

The researcher-scientist might speak thus: I fully recognize that the major contributions to the theory and practice of psychotherapy are based on careful observations of perceptive clinicians. Freud and those who followed in his footsteps have revolutionized our thinking about psychic conflicts and developed a unique method for effecting fundamental and lasting personality change. Certainly, clinical observation and painstaking efforts by individual therapists have taught us most of what we know about the principles of psychotherapy and psychotherapeutic technique. It is possible that these insights will not be equaled or even approached in another century. However, we must not lose sight of the fact that Freud considered psychoanalysis a *science*, and he strove until his dying day to preserve the scientific attitude which formed the basis of his most notable discoveries. Unlike some of his followers, he did not consider his formulations as final pronouncements or immutable truths. To be sure, his methods were relatively uncontrolled, nor was he able to marshal the scientific rigor that is demanded of investigations today; but he applied the scientific method as clearly and unmistakably as an experimental psychologist who runs rats through a maze or a physiologist who measures the contractions of a muscle. The differences are quantitative rather than qualitative.

Furthermore, it must be recognized that our knowledge of psychotherapy is at a very rudimentary stage. As Kubie (1956, p. 102) points out, we stand at the threshold of a new era; discoverers like Freud have pointed the way, but it remains for others to systematize his insights, to document the wealth of his observations, to sift, discard, build, and revamp. Reluctant as the clinician may be to bring the searchlight of dispassionate and objective research to bear upon his clinical operations, no other

course is open. It may be a long time before our knowledge is reorganized in revolutionary ways, and, compared to the sweeping discoveries of Freud, the patient and laborious investigations of the researcher may appear pedestrian, obsessive, and even picayune. Nevertheless, they are needed and may surely be counted upon to advance the frontiers of knowledge. Possibly none or very few of the current research investigations provide principles which *radically* change therapeutic technique or even have a marked influence on clinical practice. But neither have clinical insights during the last decades led to momentous changes in therapeutic technique. Progress in this area has been slow, but by no means negligible. Besides, the clinician's attitude of respect, empathy, and understanding has nothing to do with what research has to contribute; on the contrary, greater specificity which will come from research will strengthen and illuminate the clinical framework of his practical operations and gradually enable him to be of better help to his patients.

These arguments, based as they often are on emotional commitments, have a familiar ring. They underlie the nomothetic-ideographic controversy; they are mirrored in the fundamental differences between the *geisteswissenschaftliche* approach of German psychology and the naturalistic-positivistic approach of American psychology; they have recently been revived in existentialist thinking and *Daseinsanalyse* (May, Angel, & Ellenberger, 1958); and they are rooted in the subject-object schism of Western philosophy. These basic philosophic convictions determine one's view of psychology as a science, the methods by which knowledge is acquired about psychological processes, as well as one's conception of psychotherapy and of the role and function of the psychotherapist.

It is instructive to note that after having lived with Freud's discoveries for half a century, and viewed the individual patient as the battleground of impersonal forces shaping his destiny, we are still largely ignorant of the nature of the therapeutic action, and renewed attention is being focused on the psychotherapist as a *person*, who—some suspect—plays a larger part in the therapeutic endeavor than has hitherto been acknowledged or made explicit.[5] Working with widely different methods of investigation, theoretical orientations, and therapeutic objectives, numerous investigators appear to converge on this point. Alexander, after a lifetime of experience in psychoanalysis, refers to "the most opaque area of psychoanalysis, the question of the therapist's influence on the treatment process by the virtue of being what he is: an individual personality, distinct from all other therapists." (1958, p. 311). Frank, speaking as a researcher concerned with experimental controls, sees as a chief problem "to distinguish the effects

[5] In one of his last papers Freud makes this highly significant point: "Amongst the factors which influence the prospects of an analysis and add to its difficulties in the same manner as the resistances, we must reckon not only the structure of the *patient's* ego but the personal characteristics of the *analyst*. . . . The analyst. . . , because of the peculiar conditions of his work, is really impeded by his own defects in his task of discerning it in a manner conducive to cure. So there is some reason in the demand for a comparatively high degree of psychical normality and correct adjustment in the analyst as one of his qualifications for his work. And there is another point: he must be in a superior position in some sense if he is to serve as a model for his patient in certain analytic situations and, in others, to act as his teacher. Finally, we must not forget that the relationship between analyst and patient is based on a love of truth, that is, on the acknowledgment of reality, and that it precludes any kind of sham or deception." (Freud, 1952, pp. 351–352). As in earlier papers, the emphasis rests on those aspects of the therapist's personality which might create an impediment to therapy; however, Freud also recognizes that the therapist must be "in a superior position in some sense if he is to serve as model for his patient. . . ." Unfortunately, one is left to guess what is meant by "superior position," but I wonder whether Freud meant that the therapist had to be somewhat of a *Persönlichkeit*. If in the passage immediately following he expressed doubts that the candidate would acquire the "ideal qualifications" for his work *solely* in his personal analysis, he clearly had the removal of "blind spots" in mind; but perhaps he also questioned whether the candidate could ever become a *Persönlichkeit solely* through his personal analysis unless his analyst in turn embodied the "model qualities" which Freud alluded to.

of the therapist's personality or attitude, from the effects of his technique." (1959). And further: "The most important, and unfortunately the least understood, situational variable in psychotherapy is the therapist himself. His personality pervades any technique he may use, and because of the patient's dependence on him for help, he may influence the patient through subtle cues of which he may not be aware." (Frank, 1959). Elsewhere Frank considers the possibility that the patient's faith and trust in the therapist may be the single most important variable in therapeutic change (1958). Rogers speaks of the therapist's "genuineness in the relationship," his "unconditional positive regard," and "empathy." He views the therapeutic relationship "as a heightening of the constructive qualities which often exist in part in other relationships, and an extension through time of qualities which in other relationships tend at best to be momentary" (1957, p. 101). In the same paper, Rogers comes to the conclusion that "the techniques of the various therapies are relatively unimportant" (1957, p. 102) and, stressing the overriding importance of the therapist's attitudes:

> "Any of the techniques may communicate the fact that the therapist is expressing one attitude at a surface level, and another contradictory attitude which is denied to his own awareness. Thus one value of such a theoretical formulation as we have offered is that it may assist therapists to think more critically about those elements of their experience, attitudes, and behaviors which are essential to psychotherapy, and those which are nonessential or even deleterious to psychotherapy" (1957, p. 103).

The present writer, basing his conclusion on a study of psychotherapists in an experimental situation, regards the therapist's contribution to the treatment process as both personal and technical:

> "His personal attributes (maturity, warmth, acceptance, etc.) enable him to create the kind of interpersonal relationship in which constructive personality change can take place; his knowledge of psychodynamic principles permits him, in and through this relationship, to initiate the kinds of emotional unlearning and learning experiences that are considered necessary to the alleviation or resolution of neurotic conflicts. The latter would be impossible without the former; the former, by itself, would never be sufficient." (Strupp, 1958, p. 66).

There appears to be, then, a growing disenchantment with psychotherapy as a "technique," American preoccupations with technology notwithstanding. The keynote of this development was perhaps sounded by Freud himself. His attention to the technical aspects of psychoanalytic therapy declined toward the middle and end of his career, and contrary to his theoretical revisions in other areas he had relatively little to add to his earlier papers on technique. While he considered the therapist's personal attributes important, he stressed their potentially noxious influences, leaving the positive aspects largely unformulated (Freud, 1952). True, at one time he conceived of psychoanalysis as a kind of "after-education" but later on expressed a distaste for the conception of the therapist as an "educator" or "mentor." (See, in this connection, Burchard, 1958.) He feared that the therapist might make similar mistakes as the patient's parents in "indoctrinating" him with a set of values and rules which would impede his free development and self-direction. In the 1920s and '30s much emphasis was given to interpretations, their timing, correctness, and adequacy. Concomitantly, the person making the interpretations tended to be lost sight of, and under Freud's impetus the essence of psychoanalytic therapy was seen as the resolution of the transference neurosis through appropriate interpretations—a process, one surmised, which would reel off, regularly, automatically, and inexorably, if but the "correct" technical procedures were followed. This is not the place to trace subsequent developments except to draw attention to a trend culminating in the "rediscovery" of the therapist as a person, whose attitudes, personality, and values presumably play an exceedingly important,

if not crucial, part in the therapeutic process. Concomitantly, it is being realized that increasing effort must be expended to elucidate his personal influence, which may far transcend his technical contribution.[6]

Could it be that we have come full circle? Starting from the age-old view of the potentially beneficial effects of love, friendship, and acceptance of one's emotional well-being, we seem to have traversed an era in which a highly technical procedure (psychoanalysis) came to the ascendency. Here the therapeutic task was conceived as an effort to bring about a reconciliation of impersonal forces—to help the ego come to terms with an irrational id and an unreasonable, overly severe superego. Moving along a spiral, the developments of ego psychology have stressed a conception in which the ego's autonomy and integrative functions again came to the fore. Contemporaneously, the psychotherapeutic situation is again seen as an encounter between two *persons*. The patient is regarded as an individual with problems in living, but also as a human being striving to realize his potentialities. The therapist is viewed, not as an analytic manipulator or technician, but as a human being who, aided by technical training *and* experience in living, attempts to *understand* the patient as a person. This understanding is to be accomplished through emotional participation in living with the other person over an extended period of time, to the end that the therapist's influence conduces to constructive personality growth. The focus is upon the experiential components of the situation, to which everything else becomes subordinate, including the technical operations by which the experience is facilitated. Consequently, the operations of psychotherapy are not simply to be explained by rules which tell the novice when to interpret and what; rather they presuppose a deep understanding of the principles governing the whole gamut of human collaboration. Aware of this complexity, though speaking in a different context, Freud said:

> "Anyone who hopes to learn the noble game of chess from books will soon discover that only the openings and endgames admit of an exhaustive systematic presentation and that the infinite variety of moves which develop after the opening defy any such description. This gap in instruction can only be filled by a diligent study of games fought out by masters." (1955, Vol. XII, p. 123).

This analogy illustrates a point, but like all analogies it has its limitations. It asserts that (a) there are rules governing the conduct of psychotherapy; (b) these rules are explicit and communicable, hence teachable; (c) there is an over-all strategy in psychotherapy as there is in chess, i.e., there is a specific objective; (d) while knowledge of the basic rules is a prerequisite, it does not, in and of itself, enable one to play the game with any degree of skill; in other words, the general rules must be applied in highly specific and concrete situations, and once a measure of skill is acquired, the basic rules seem to recede to the background, so that the player is largely unaware that he is employing them; (e) one learns the skill by watching and studying closely the operations of experts; in doing so, one learns to avoid pitfalls and refines his technique which ultimately becomes interwoven with one's personality. One of the important shortcomings of this analogy is that the rules of psychotherapy are established empirically, not, as in chess, a priori. Furthermore, they are modifiable in the light of empirical findings, that is, their validity is to be established by reference to an external criterion. As the analogy suggests, the expert is more "successful" in playing the game than a novice, as evidenced by the fact that he wins more games. Is the expert psychotherapist more "successful" than the novice? This is a purely empirical question, which so far as I know has not been answered to anyone's satisfaction. One of the important reasons that it has not been done is the difficulty of *comparing* the performance of an expert with that of a novice, under rigidly specified conditions, and to note differences in outcome. Such a comparison presupposes the definition and measurement of dimensions which are

[6] For a more extended discussion of this point, see Strupp, in press.

hypothesized to make a difference (maturity, technical skill, for example). In all probability, the expert does differ in his therapeutic performance from the novice, but this must be demonstrated unequivocally, beyond a reasonable doubt. One approach might be to study the therapeutic behavior of a group of experts to see what they have in common. One might then search for the same common characteristics (therapeutic ingredients) in a group of beginners and determine whether differences exist. If so, they should lead to predictably different outcomes. If not, the results may be due to other factors yet to be defined. Freud attributes the results of psychoanalysis primarily to certain technical operations on the therapist's part; Rogers is inclined to search for them in the area of the therapist's "unconditional regard"; Frank hypothesizes a series of nonspecific factors, such as faith and trust. I suspect that technical skill and salient characteristics of the therapist's personality interact to make possible a new emotional experience, cognitive learning, and the gradual acquisition of new attitude patterns.

Preoccupations with psychotherapeutic technique have increasingly come under criticism in recent years. In this country, Rogers has been an eloquent spokesman for the view that *understanding* the patient is the therapist's first and foremost task. In Europe, the rise of existential psychotherapy has brought with it a further deemphasis of technique. In explicating this point of view to American readers, May states: "Existential analysis is a way of understanding human existence, and its representatives believe that one of the chief (if not *the* chief) blocks to the understanding of human beings in Western culture is precisely the overemphasis on technique, an overemphasis which goes along with the tendency to see the human being as an object to be calculated, managed, 'analyzed'" (May et al., 1958, p. 76). The central issue is "whether the human being is an object to be analyzed or a being to be understood." (May et al., 1958, p. 81). However, the emphasis on the here-and-now relationship neither obviates technical training nor should it discourage research on problems of technique. In May's words, this relationship

> "... is in no way an over-simplification or short cut; it is not a substitute for discipline or thoroughness of training. It rather puts these things in their context—namely, discipline and thoroughness of training directed to understanding human beings as human. The therapist is assumedly an expert; but, if he is not first of all a human being, his expertness will be irrelevant and quite possibly harmful. The distinctive character of the existential approach is that understanding *being human* is no longer just a 'gift,' an intuition, or something left to chance; it is the 'proper study of man,' in Alexander Pope's phrase, and becomes the center of a thorough and scientific concern in the broad sense. The existential analysts do the same thing with the structure of human existence that Freud did with the structure of the unconscious—namely, take it out of the realm of the hit-and-miss gift of special intuitive individuals, accept it as the area of exploration and understanding, and make it some extent teachable." (1958, pp. 82–83).

Thus, it is of little avail to investigate whether psychotherapy "works," or even whether one technique is more "effective" than another. Rather one must ask: Is this particular therapist, by virtue of being a particular person, capable of creating the kinds of conditions in which a given technique or techniques can attain their maximum usefulness? The question of the relative effectiveness of techniques still has to be answered, but the primary focus is on the *person* of the therapist by whom a particular technique is used. Can he understand the patient as a human being struggling with life's problems? Can he communicate this understanding, to the end that the patient feels deeply understood? For methodological reasons, research may emphasize one aspect or the other, but the totality of therapist and technique in interaction with a particular patient cannot be ignored.

There appears to be a serious danger in overemphasizing either relationship or tech-

nique at the expense of the other. To avoid these pitfalls it would appear that ways and means must be found to conduct investigations which do justice both to the demands of scientific rigor and the depth and breadth of the therapeutic undertaking. To this end, researchers must become better and more insightful clinicians, and clinicians must develop a greater awareness of the ideals which the scientist espouses. Among other things, such a rapprochement may result in a larger number of research contributions which are meaningful and relevant to the therapist. Similarly, it may inspire that tentativeness, caution, and respect for error in human observation which is still seen lacking in many clinicians.

The subject-object split which still pervades the science of psychology seems a particularly grave obstacle to the interpenetration of clinical practice and research in psychotherapy. Russell (1948), quoted by Szasz (1957), has eloquently dealt with this issue and defined psychology as a science dealing with essentially private experience. Among psychologists, Allport (1955) has taken issue with the prevailing behaviorist tendency to regard the human person as "empty." In the area of psychotherapy, the investigations of Rogers and his students are perhaps the best illustration that fruitful research on the process of psychotherapy can be done, while retaining something of the uniqueness of the persons participating in the process (Rogers & Dymond, 1954). These are but beginnings, but they seem important beginnings in studying objectively the subjective experiences of patient and therapist in interaction.

Erwin Straus's dictum that "Whatever is related to my particular existence lessens and obscures knowledge" (May et al., 1958, p. 145) seems to strike at the core of an important dilemma in research on psychotherapy. In order to expand scientific knowledge of the therapeutic process, it is necessary to objectify essentially subjective experiences, but as one succeeds in doing so, one runs the danger of sacrificing the essence of what one is studying. Therefore, the obverse of the quotation likewise appears to be true: Knowledge lessens and obscures whatever is related to my particular existence. This, of course, is precisely what science attempts to do in all areas of investigation. Is psychotherapy an exception? If so, it can hardly become a science. If not, the search for invariance amidst change must go forward; common elements must be abstracted; and the unique aspects of the therapeutic encounter may have to go by the board. In one sense, I seem to have suggested that the future *may* bring the development of techniques for *simultaneously* achieving both objectives. This is a hope which may never materialize. We may find that we can't have our cake and eat it. In that event, the therapist and the researcher in psychotherapy may move further apart, unless the clinician abandons his commitments, which seems to be as doubtful as that the researcher will forsake the ideals of science.

Among other things, it will be fruitful to learn how a particular patient is perceived, experienced, and reacted to by a particular therapist and *vice versa*. But if "objective" knowledge in psychotherapy is possible we must also learn more about what patient and therapist are "really" like, as seen by *external* observers. There is reason to suspect that preconceptions on the therapist's part (e.g., notions about the treatability or nontreatability of certain "conditions") may blind him to the patient's potentialities and assets (Strupp, 1958). Of course, the therapist should develop maximal self-awareness of all factors which might adversely affect his therapeutic attitudes. But, important as the study of subjective elements and biasing factors may be, it must not overshadow the thorough scrutiny of interpersonal processes (e.g., transference manifestations) qua objective facts which, as every analyst knows, usually emerge in the treatment process irrespective of the therapist's unique personality characteristics even though they may be colored by the latter. And—to continue on this side of the fence—it seems that the latter-day emphasis on "encounter," "here-and-now experience," "understanding," etc. does scant justice to the painstaking and prolonged process of "working through"—a highly *technical*

operation—which is considered an integral part of all fundamental personality change achieved through intensive psychotherapy. Difficult as controlled research on these problems will be, I am sure the limits have not been tested. However, I am not fully convinced that it will be possible to reconcile successfully scientific rigor with the richness and subtle complexity of interpersonal dynamics.

CONCLUSION

The questions raised at the beginning of this paper still loom large. It is disquieting to contemplate the large discrepancy between clinical and philosophical insights on the one hand, and research accomplishments, on the other. As researchers we seem to lack methods for making greater inroads on the phenomena with which psychotherapy deals—the broad spectrum of human experience. For instance: How do we assess and measure such qualities in the therapist as: respect for the patient's struggle toward self-realization and self-direction; capacity for empathy; warmth; acceptance of the human-ness of another person; depth of one's *Weltanschauung* and life experience; emotional maturity; ability to serve as a model of reality; and so forth[7]—all of which undoubtedly play an important role in determining the extent to which the therapist can participate in and collaborate with the patient's striving for realizing his human potentialities? By contrast, the quantitative and comparative analyses of technique, formidable as they are, appear like child's play.

The fact that techniques for measuring significant personality attributes are in their infancy does not mean that they are doomed to remain there. The fact that so far the contributions from researchers to psychotherapeutic theory and practice have been relatively slight need not remain an immutable truth and omen for the future. The task poses a challenge to our imagination as researchers. We must show greater penetration in forging our research tools and refuse to purchase precision in measurement at the expense of shallowness of concepts. If we agree to the proposition that psychotherapy's future is that of a scientific discipline, we have no choice but to undertake the laborious and painful drudgery of checking the empirical value of brilliant clinical insights glimpsed by intrepid pioneers and to sharpen our research instruments that they may become adequate to deal with the phenomena in our domain. It is just barely possible that a few crumbs of insight left over by the giants may be the reward of the patient researcher, not to mention the gratification of demolishing along the way some hypotheses which contemporaneously enjoy the status of a creed.

[7] We need to specify those qualities of character which give the therapist stature as a mature personality and model of reality. Compare reference footnote 5 for a statement of Freud's position.

REFERENCES

Alexander, F. Unexplored areas in psychoanalytic theory and treatment. *Behav. Sci.*, 1958, 3, 293–316.

Allport, G. W. *Becoming: Basic considerations for a psychology of personality.* New Haven: Yale University Press, 1955.

Burchard, E. M. L. The evolution of psychoanalytic tasks and goals. *Psychiatry*, 1958, 21, 341–357.

Frank, J. D. Problems of controls in psychotherapy as exemplified by the psychotherapy research project of the Phipps Psychiatric Clinic. In E. A. Rubinstein & M. B. Parloff (Eds.), *Research in psychotherapy.* Washington, D. C.: Amer. Psychol. Assn., 1959. Pp. 10–26.

Frank, J. D. Some effects of expectancy and influence in psychotherapy. In J. H. Masserman & J. L. Moreno (Eds.), *Progress in psychotherapy, Vol. III.* New York: Grune & Stratton, 1958. Pp. 27–43.

Freud, S. Analysis terminable and interminable. In *Collected Papers, Vol. V.* London: Hogarth Press, 1952. Pp. 313–357.

Freud, S. Further recommendations in the technique of psychoanalysis. In *Complete Psychological Works of Sigmund Freud, Standard Edition.* London: Hogarth Press, 1955.

Kardiner, A. New horizons and responsibilities of psychoanalysis. *Amer. J. Psychoanalysis*, 1958, 18, 115–126.

Kubie, L. S. Some unsolved problems of psychoanalytic psychotherapy. In Frieda Fromm-Reichmann & J. L. Moreno (Eds.), *Progress in psychotherapy 1956.* New York: Grune & Stratton, 1956. Pp. 87–102.

May, R., Angel, E., & Ellenberger, H. F., (Eds.).

Existence: A new dimension in psychiatry and psychology. New York: Basic Books, 1958.

Rogers, C. R. Persons or science: A philosophical question. *Amer. Psychologist*, 1955, 10, 267–278.

Rogers, C. R. The necessary and sufficient conditions of therapeutic personality change. *J. consult. Psychol.*, 1957, 21, 95–103.

Rogers, C. R. & Dymond, Rosalind F. (Eds.), *Psychotherapy and personality change*. Chicago: University of Chicago Press, 1954.

Russell, B. *Human knowledge: Its scope and limits*. New York: Simon & Schuster, 1948. Ch. 6.

Sears, R. R. *Survey of objective studies of psychoanalytic concepts*. New York: Social Science Research Council, 1943.

Strupp, H. H. The psychotherapist's contribution to the treatment process. *Behav. Sci.*, 1958, 3, 34–67.

Strupp, H. H. Toward an analysis of the therapist's contribution to the treatment process. *Psychiatry*, in press.

Szasz, T. S. *Pain and pleasure: A study of bodily feelings*. New York: Basic Books, 1957. Ch. 2.

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These sensations are projected by the mind so as to clothe appropriate bodies in external nature. Thus the bodies are perceived as with qualities which in reality do not belong to them, qualities which in fact are purely the offspring of the mind. Thus nature gets credit which should in truth be reserved for ourselves: the rose for its scent; the nightingale for his song; and the sun for its radiance. The poets are entirely mistaken. They should address their lyrics to themselves, and should turn them into odes of self-congratulation on the excellency of the human mind. Nature is a dull affair, soundless, scentless, colourless; merely the hurrying of material, endlessly, meaninglessly.

ALFRED NORTH WHITEHEAD, *Science and the Modern World*

When one says "John is a more rational person than Henry," many meanings of the word "rational" can be distinguished. Some of these meanings are related to affairs of everyday life, some to affairs of scientific theorizing, and it is the scientific rationalities that writers on social organization and decision making commonly refer to as features of "rational choice." It is here suggested that other meanings of rationality should be taken into account and treated as data in empirical research.

THE RATIONAL PROPERTIES OF SCIENTIFIC AND COMMON SENSE ACTIVITIES*

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INTRODUCTION

THE program of his discipline requires that the sociologist scientifically describe a world that includes as problematical phenomena not only the other person's actions, but the other person's knowledge of the world. As a result, the sociologist cannot avoid *some* working decision with respect to the various phenomena intended by the term "rationality."

Commonly, sociological researchers decide on a definition of rationality by selecting one or more features from among the properties of scientific activity as it is ideally described and understood.[1] The definition is then used methodologically to aid the researcher in deciding the realistic, pathological, prejudiced, delusional, mythical, magical, ritual, and similar features of everyday conduct, thinking and beliefs.

But because sociologists find with such overwhelming frequency that effective, persistent, and stable actions and social structures occur despite obvious discrepancies between the lay person's and the ideal scientist's knowledge and procedures, they have found that the rational properties which their definitions discriminated are empirically uninteresting. They have preferred instead to study the features and conditions of non-rationality in human conduct. The result is that in most of the available theories of social action and social structure rational actions are assigned residual status.

With the hope of correcting a trend, it is the purpose of this paper to remedy this residual status by re-introducing as a problem for empirical inquiry (a) the various rational properties of conduct, as well as (b) the conditions of a social system under which various rational behaviors occur.

RATIONAL BEHAVIORS

"Rationality" has been used to designate many different ways of behavior. A list of such behaviors can be made without treating any one as definitive of the term "rationality." Alfred Schutz' classical paper on the problem of rationality (1943) inventories these meanings and is our point of departure.

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[1] One definition that enjoys current favor is known as the rule of empirically adequate means. A person's actions are conceived by the researcher as steps in accomplishing tasks whose possible and actual accomplishment is empirically decidable. Empirical adequacy is then defined in terms of the rules of scientific procedure and the properties of the knowledge that such procedure produces.

When the various meanings of the term which Schutz inventoried are phrased as descriptions of conduct, the following list of behaviors results. In the remainder of the paper, these behaviors will be referred to as "the rationalities."

1. Categorizing and comparing. It is commonplace for a person to search his experience for a situation with which to compare the one he addresses. Sometimes rationality refers to the *fact* that he searches the two situations with regard to their comparability, and sometimes to his *concern* for making matters comparable. To say that a person addresses the tasks of comparison is equivalent to saying that he treats a situation or a person or a problem as an instance of a type. Thereby the notion of a "degree of rationality" is encountered. The extensiveness of a person's concern with classification, the frequency of this activity, the success with which he engages in it are frequently the behaviors meant by saying that one person's activities are more rational than another's.

2. Tolerable error. It is possible for a person to "require" varying degrees of "goodness of fit" between an observation and a theory in terms of which he names, measures, describes, or otherwise intends the sense of his observation as a datum. He may pay a little or a lot of attention to the degree of fit. On one occasion he will allow a literary allusion to describe what has occurred. On another occasion and for the same occurrences he may search the mathematical journals for a model to order them. It is sometimes said, then, that one person is rational while another is not or is less so, when that person pays closer attention than does his neighbor to the degree of fit between what he has observed and what he intends as his finding.

3. Search for "means." Rationality is sometimes used to mean that a person reviews rules of procedure which in the past yielded the practical effects now desired. Sometimes it is the fact that a person seeks to transfer rules of practice which had a payoff in situations of like character; sometimes it is the frequency of this effort; at other times the rational character of his actions refers to the person's ability or inclination to employ in a present situation techniques that worked in other situations.

4. Analysis of alternatives and consequences. Frequently the term rationality is used to call attention to the fact that a person, in assessing a situation, anticipates the alterations which his actions will produce. Not only the fact *that* he "rehearses in imagination" the various courses of action which will have occurred; but the care, attention, time, and elaborateness of analysis paid to alternative courses of action are frequent references. With respect to the activity of "rehearsing in imagination" the competing lines of actions-that-will-have-been-completed, the clarity, extent of detail, the number of alternatives, the vividness and the amount of information which fills out each of the schemata of competing lines of action are often the intended features in calling a person's actions "rational."

5. Strategy. Prior to the actual occasion of choice a person may assign to a set of alternative courses of action the conditions under which any one of them is to be followed. Von Neumann and Morgenstern (1947) have called the set of such decisions a player's strategy. The set of such decisions can be called the strategy character of the actor's anticipations. A person whose anticipations are handled with the belief that his circumstances tomorrow will be like those he has known in the past is sometimes said to be acting with less rationality than the one who addresses alternatively possible future states of his present situation by the use of a manual of "what-to-do-in-case-of's."

6. Concern for timing. When we say that a person intends through his behaviors to realize a future state of affairs, we frequently mean that the person entertains an expectation of the scheduling of events. The concern for timing involves the extent to which he takes a position with regard to the possible ways in which events can occur temporally. A definite and restricted frame of scheduled possibilities is compared with a "lesser rationality" that consists of the person orienting the future fall of events under the aspect of "anything can happen."

7. Predictability. Highly specific expecta-

tions of time scheduling can be accompanied by a concern with the predictable characteristics of a situation. A person may seek preliminary information about the situation in order to establish some empirical constants, or he may attempt to make the situation predictable by examining the logical properties of the constructs he uses in "defining" it or by reviewing the rules that govern the use of his constructs. Accordingly, making the situation predictable means taking whatever measures are possible to reduce "surprise." Both the desire for "surprise in small amounts" and the use of whatever measures yield it are frequently the behaviors intended by the term rationality.

8. Rules of procedure. Sometimes rationality refers to rules of procedure and inference in terms of which a person decides the correctness of his judgments, inferences, perceptions and characterizations. Such rules define the distinct ways in which a thing may be *known*—distinctions, for example, between fact, supposition, evidence, illustration, and conjecture. For our purposes two important classes of such rules of correct decisions may be distinguished: "Cartesian" rules and "tribal" rules. Cartesian rules propose that a decision is correct because the person followed the rules without respect for persons, i.e., that the decision was made without regard for social affiliation. By contrast, "tribal" rules provide that a decision is or is not correct according to whether certain interpersonal solidarities are respected as conditions of the decision. The person counts his decision right or wrong in accordance with whom it is referentially important that he be in agreement.

The term rationality is frequently used to refer to the application of Cartesian rules of decision. Because conventions may impose constraints on such decision making, the extent to which the constraints are suppressed, controlled, or rendered ineffective or irrelevant is another frequent meaning of rationality.

9. Choice. Sometimes the fact that a person is aware of the actual possibility of exercising a choice and sometimes the fact that he chooses, are popular meanings of rationality.

10. Grounds of choice. The grounds upon which a person exercises a choice among alternatives as well as the grounds he uses to legitimize a choice are frequently pointed out as rational features of an action. Several different behavioral meanings of the term "grounds" need to be discriminated.

(a) Rational grounds sometimes refer exclusively to the scientific *corpus*[2] of information as an inventory of propositions which is treated by the person as correct grounds of further inference and action.

(b) Rational grounds sometimes refer to such properties of a person's knowledge as the "fine" or "gross" structure of the characterizations he uses, or whether the "inventory" consists of a set of stories as compared with universal empirical laws, or the extent to which the materials are codified, or whether the *corpus* in use accords with the *corpus* of scientific propositions.

(c) Insofar as the grounds of choice are the strategies of action, as was noted before in point 5, another sense of rationality is involved.

(d) Grounds of a person's choice may be those which he quite literally *finds* through retrospectively interpreting a present outcome. For example, in the effort to determine what was "really" decided at a prior time, a person may realize such grounds in the course of historicizing an outcome. Thus, if a present datum is treated as an-answer-to-some-question, the datum may motivate the origina! question. Selecting, arranging, and unifying the historical context of an action after its occurrence so as to present a publicly acceptable or coherent account of it is a familiar meaning of "rationalization."

11. Compatibility of ends-means relationships with principles of formal logic. A person may treat a contemplated course of action as an arrangement of steps in the solution of a problem. He may arrange these steps as a set of "ends-means" relationships but count the problem solved only if these relationships are accomplished without violating the ideal of full compatibility with the principles of formal scientific logic and the rules of

[2] The concept of the *corpus* of knowledge is taken from the work of Felix Kaufmann (1944, pp. 33–66).

scientific procedure.[3] The fact that he may do so, the frequency with which he does so, his persistence in treating problems in this way, or the success that he enjoys in following such procedures, are alternative ways of specifying the rationality of his actions.

12. Semantic clarity and distinctness. Reference is often made to a person's attempt to treat the semantic clarity of a construction as a variable with a maximum value which must be approximated as a required step in solving the problem of constructing a credible definition of a situation. A person who withholds credence until the condition of approximate maximum value has been met is frequently said to be more rational than another who will lend credence to a mystery.

A person may assign a high priority to the tasks of clarifying the constructs which make up a definition of a situation and of deciding the compatibility of such constructs with meanings intended in terminologies employed by others. On the other hand, a person may pay little attention to such tasks. The former action is sometimes said to be more rational than the latter.

13. Clarity and distinctness "for its own sake." Schutz points out that a concern for clarity and distinctness may be a concern for distinctness that is adequate for the person's purposes. Different possible relationships, ideal or actual, between (a) a concern for clarity and (b) the purposes which the clarity of the construct serves reveal additional behavioral meanings of rationality. Two variables are involved: (1) how highly one esteems clarification for its own sake, and (2) the value assigned by the person to the accomplishment of a project. One relationship between these variables makes the task of clarification itself the project to be accomplished. This is the meaning of "clarification for its own sake." But the relationship between the two variables may be treated by a person as consisting in some degree of independent variability. Such a relationship would be meant when treating as an ideal "clarification that is sufficient for present purposes." Rationality frequently means a high degree of dependence of one upon the other. Such a dependence when treated as a rule of investigative or interpretive conduct is sometimes meant in the distinction between "pure" and "applied" research and theory.

14. Compatibility of the definition of a situation with scientific knowledge. A person can allow what he treats as "matters of fact" to be criticized in terms of their compatibility with the body of scientific findings. The "allowed legitimacy of such criticism" means that in the case of a demonstrated discrepancy between what the person treats as correct grounds of inference (the meaning of "fact") and what has been demonstrated as correct, the former will be changed to accommodate to the latter. Thus a person's actions are said to be rational to the extent that he accommodates or is prepared to accommodate to what is scientifically the case.

Frequently rationality refers to the person's feelings that accompany his conduct, e.g. "affective neutrality," "unemotional," "detached," "disinterested," and "impersonal." For the theoretical tasks of this paper, however, the fact that a person has such feelings is uninteresting. It is of interest, however, that a person uses his feelings about his environment to recommend the sensible character of the thing he is talking about or the warrant of a finding. There is nothing that prohibits a scientific investigator from being passionately hopeful that his hypothesis will be confirmed. He is prohibited, however, from using his passionate hope *or* his detachment of feeling to recommend the sense or warrant of a proposition. A person who treats his feelings about a matter as irrelevant to its sense or warrant is sometimes said to be acting rationally, while a person who recommends sense and warrant by invoking his feelings is said to act with less rationality. This holds, however, only for ideally described scientific activities.

SCIENTIFIC RATIONALITIES

The foregoing rationalities may be used to construct an image of a person as a type of behavior. A person can be conceived who

[3] When treated as a rule for defining descriptive categories of action, this property is known as the rule of the empirical adequacy of means.

may[4] search a present situation for its points of comparability to situations that he knew in the past and may search his past experience for formulas that have yielded the practical effects he now seeks to bring about. In this task he may pay close attention to these points of comparability. He may anticipate the consequences of his acting according to the formulas that recommend themselves to him. He may "rehearse in imagination" various competing lines of action. He may assign to each alternative, by a decision made prior to the actual occasion of choice, the conditions under which any one of the alternatives is to be followed. Along with such structurings of experience as these, the person may intend through his behaviors to realize a projected outcome. This may involve his paying specific attention to the predictable characteristics of the situation that he seeks to manipulate. His actions may involve the exercise of choice between two or more means for the same ends or of a choice between ends. He may decide the correctness of his choice by invoking empirical laws. And so on.

In extending the features of this behavioral type to incorporate all of the preceding rationalities, a distinction arises between the interests of everyday life and the interests of scientific theorizing. Where a person's actions are governed by the "attitude of daily life," all of the rationalities can occur *with four important exceptions.* Phrased as ideal maxims of conduct, these excepted rationalities state that the projected steps in the solution of a problem or the accomplishment of a task, i.e., the "means-ends relationships," be constructed in such a way (1) that they remain in full compatibility with the rules that define scientifically correct decisions of grammar and procedure; (2) that all the elements be conceived in full clearness and distinctness; (3) that the clarification of both the body of knowledge as well as the rules of investigative and interpretive procedure be treated as a first priority project; and (4) that the projected steps contain only scientifically verifiable assumptions that have to be in full compatibility with the whole of scientific knowledge. The behavioral correlates of these maxims were described before as rationalities 11 through 14. For ease of reference I shall refer to these four as "the scientific rationalities."

It is the crux of this paper and of a research program that would follow if its arguments are correct, that *the scientific rationalities, in fact, occur as stable properties of actions and as sanctionable ideals only in the case of actions governed by the attitude of scientific theorizing. By contrast, actions governed by the attitude of daily life are marked by the specific absence of these rationalities either as stable properties or as sanctionable ideals.* Where actions and social structures governed by the presuppositions of everyday life are concerned, attempts to stabilize these features or to compel adherence through socially systematic administration of rewards and punishments are the operations required to multiply the anomic features of interaction. All of the other rationalities, 1 through 10, however, can occur in actions governed by either attitude, both as stable properties and sanctionable ideals.

The preceding assertions are meant as empirical matters not as doctrinal ones. The reconstruction of the "problem of rationality"[5] proposed by this paper depends upon the warranted character of these assertions. Their test depends upon a viable distinction between the "attitude of daily life" and the "attitude of scientific theorizing." It is necessary, therefore, that the different presuppositions that make up each attitude be

[4] By "may" is meant available as one of a set of alternatives. It does not mean likelihood.

[5] For the sociological theorist, the "problem of rationality" can be treated as consisting of five tasks: (1) clarifying the various referrents of the term "rationality" which includes stating the behavioral correlates of the various "meanings" of rationality as (a) the individual's actions as well as (b) the "system's" characteristics; (2) deciding on the ground of the examination of experience rather than by an election of theory which of the behavioral designata go together; (3) deciding an allocation of behavioral designata between definitional and empirically problematical status; (4) deciding the grounds for justifying any of the many possible allocations that he may finally choose to make; and (5) showing the consequences of alternative sets of decisions for sociological theorizing and investigation.

briefly compared. After this is done, we shall return to the main thread of the argument.

PRESUPPOSITIONS OF THE TWO ATTITUDES

The attitudes of daily life and scientific theorizing[6] were described by Alfred Schutz in his studies of the constitutive phenomenology of common sense situations (1943; 1944; 1945; 1951; 1953). Because the arguments of this paper depend upon the assumption that these attitudes do not shade into each other, it is necessary that the presuppositions that comprise each be briefly compared.

1. The first assumption Schutz refers to is the "epoche" of the natural attitude. He shows that in everyday situations the "practical theorist" achieves an ordering of events while seeking to retain and sanction the presupposition that the objects of the world are as they appear. The person coping with everyday affairs seeks an interpretation while holding a line of "official neutrality" toward the interpretive rule that one may doubt that the objects of the world are as they appear. Stated as a property of events, this assumption consists of the expectation that a relationship of undoubted correspondence exists between actual appearances of the object and the-intended-object-that-appears-in-this-determined-fashion. Out of the set of alternative relationships between the actual appearances of the object and the intended object, as for example, the relationship of *doubtful* correspondence between the two, the person expects that the presupposed undoubted correspondence is the sanctionable one, and that the other person employs the same expectancy in a more or less identical fashion.

In the activities of scientific theorizing quite a different role of interpretive procedure is used. It provides that interpretation be conducted while holding a position of "official neutrality" toward the *belief* that the objects of the world are as they appear. The activities of everyday life, of course, permit the actor's doubt that the objects are as they appear; but this doubt is in principle a doubt that is limited by "practical considerations," i.e., his doubt is limited by his respect for certain valued, more or less routine features of the social order as seen from within, that he specifically does not and will not call into question. By contrast, the activities of scientific theorizing are governed by the strange ideal of doubt that is in principle unlimited and that specifically does not recognize the normative social structures as constraining conditions.

2. Schutz speaks of a second assumption as the person's practical interest in the events of the world. An invariant feature of the presuppositions of daily life is the person's belief that events can actually and potentially affect his actions and can be affected by his actions. Under this presupposed feature of events, the accuracy of his ordering of events is assumed by the person to be tested and testable without suspending the relevance of what he knows as fact, supposition, conjecture, fantasy and the like by virtue of his bodily and social positions in the real world. For the person in daily life, events, their relationships, their causal textures are not matters of mere theoretic interest. He does not sanction the notion that in dealing with the events of his environment it is correct to address them with the interpretive rule that he knows nothing or that *all* of what he knows he knows only "until further notice." What he knows, in the way he knows it, he assumes to be an integral feature of himself as a social object. He *sanctions* his competence as a bona-fide member of the group as a condition for adequately appreciating the sense of his everyday affairs; he does not treat this competence as irrelevant.

By contrast, the interpretive rules of the attitude of scientific theorizing provide that the accuracy of a model is to be tested while suspending judgment on the relevance of what the theorizer knows by virtue of his social and bodily positions in the real world.

3. Schutz describes the time perspective of daily life. The person who is said to orient events in the attitude of everyday life accords to possible events a temporal accent

[6] To avoid misunderstanding I want to stress that the concern here is with the attitude of scientific theorizing. The attitude that informs the activities of actual scientific inquiry is another matter entirely.

which is drawn from what the person assumes is a scheme of temporal relationships that he and other persons employ in equivalent and standardized fashion. The conversation that he is having consists for him of not only the events of his stream of experience, but of what was, is, or may be said at a time that is designated by the successive positions of the hands of the clock. The conversation is not only progressively realized through a succession of realized meanings of the course of utterances, but as of any point in its course, the conversation has a time of beginning, of duration, and of termination. Its events are co-ordinate with a socially employed scheme of temporal determinations. For the person oriented to a situation in a "practical way" his interest in standard time is not like his interest when he is theorizing scientifically.

In the attitude of daily life the interest in an interpersonally validated scheme of standard time is directed to the problems such a scheme solves for the user in scheduling and co-ordinating his inner life and bodily movements to those of others. By contrast, the interest in standard time that occurs in the attitude of scientific theorizing —assuming in the first place that the scientific theorizer is concerned with matters of fact—is confined to the use of such a scheme as a means of conceiving an empirically possible world rather than as a means of scheduling his actions. Other attitudes provide alternative and contrasting ways of temporally punctuating events. For example, in the play *Ethan Frome*, the lovers' fate comes before and as a condition for appreciating the sequence of steps that led up to it. But in situations of everyday life the person assigns to events the properties of standard time relationships. He assumes, too, that the scheme of standard time is entirely a *public* enterprise, a kind of "one great clock identical for all."

4. The person assumes a particular "form of sociality." This assumption consists of the belief that some characteristic disparity exists between the image of himself that he attributes to others and the image that he has of himself, i.e., a characteristic disparity between his public and private life. He assumes, too, that alterations of this characteristic disparity remain within his autonomous control. The assumption serves as a rule whereby the everyday theorist groups his experiences with regard to what goes properly with whom. There corresponds thereby to the common intersubjective world of communication the unpublicized knowledge, concerned with matters that one person knows that he assumes others do not know. The events of everyday situations are communicated by this integral background of "meanings held in reserve." This assumption too is heavily modified in the rules that govern the actions of scientific theorizing. In the sociality of scientific *theorizing*, the theorizer is outside of a role. All matters relevant to his depiction of a possible world are public and publicizable. It is difficult to see how one might shame the scientific theorizer or make him indignant.

5. The attitude of daily life contains the assumption of a common intersubjective world of communication. The events of every day life are informed by this integral background of sense. This assumption of a common intersubjective world of communication is startlingly modified in the actions of scientific theorizing. The "relevant other persons" for the scientific theorizer (colleagues, for example) are at best forgivable instances of highly abstract "investigators" who are, in the ideal, disembodied manuals of proper ways of proceeding.

These two sets of presuppositions do not shade into each other, nor are they distinguishable in degree. Rather, passing from the use of one set to the use of the other—from one "attitude" to another—produces a radical alteration in the person's structurings of events and their relationships. In the literal mathematical sense the two attitudes produce logically incompatible sets of events. The nature of the difference between the systems of events that are constituted by the two sets of interpretive presuppositions is illustrated in the following example. The related events that a viewer witnesses on his television screen when he attends to "the story" may be compared with the events he witnesses when he attends to the scene as a set of effects accomplished by actors be-

having in accordance with instructions from a producer. It would be the grossest philosophical didacticism to say that the viewer has seen "different aspects of the same thing," or that the events of the story are "nothing but" uncritically appreciated events of the production.

METHODOLOGY

It is the scientific rationalities which writers on social organization and decision making commonly refer to as features of "rational choice." It is proposed here, however, that the scientific rationalities are neither properties of nor sanctionable ideals of choices exercised within the affairs governed by the presuppositions of everyday life. If the scientific rationalities are neither stable properties nor sanctionable ideals of everyday life, then the troubles encountered by researchers and theorists with respect to the concepts of organizational purposes, the role of knowledge and ignorance in interaction, the difficulties in handling meaningful messages in mathematical theories of communication, the anomalies found in studies of betting behavior, the difficulties in rationalizing the concept of abnormality in light of cross-cultural materials may be troubles of their own devising. The troubles would be due not to the complexities of the subject matter, but to the insistence on conceiving actions in accordance with scientific conceits instead of looking to the actual rationalities that persons' behaviors, in fact, exhibit in the course of managing their practical affairs.

Schutz (1943, pp. 142–143) tells us what it means to say that an actor has rational choice:

> "Rational choice would be present if the actor had sufficient knowledge of the end to be realized as well as the different means apt to succeed. But this postulate implies:
>
> 1. Knowledge of the place of the end to be realized within the framework of the plans of the actor (which must be known by him, too).
> 2. Knowledge of its interrelations with other ends and its compatibility or incompatibility with them.
> 3. Knowledge of the desirable and undesirable consequences which may arise as by-products of the realization of the main end.
> 4. Knowledge of the different chains of means which technically or even ontologically are suitable for the accomplishment of this end regardless of whether the actor has control of all or several of these elements.
> 5. Knowledge of the interference of such means with other ends of other chains of means including all their secondary effects and incidental consequences.
> 6. Knowledge of the accessibility of these means for the actor, picking out the means which are within his reach and which he can and may set going.
>
> The aforementioned points do not by any means exhaust the complicated analysis that would be necessary in order to break down the concept of rational choice in action. The complications increase greatly when the action in question is a social one. . . . In this case the following elements become additional determinants for the deliberation of the actor. First, the interpretation or misinterpretation of his own act by his fellow man. Second, the reaction by the other people and its motivation. Third, all the outlined elements of knowledge (1 to 6) which the actor rightly or wrongly attributes to his partners. Fourth, all the categories of familiarity and strangeness, of intimacy and anonymity, of personality and type which we have discovered in our inventory of the organization of the social world."

But, then, asks Schutz, where is this system of rational choice to be found?

> "... The concept of rationality has its native place not at the level of everyday conceptions of the social world but at the theoretical level of the scientific observation of it, and it is here that it finds its field of methodological application."

Schutz concludes that it is found in the logical status, the elements, and the uses of the model which the scientist decides on and uses as a scheme for interpreting the events of conduct. But,

> "This does not mean that rational choice does not exist within the sphere of everyday life. Indeed it would be sufficient to interpret the terms clearness and distinctness in a modified and restricted meaning, namely, as clearness and distinctness adequate to the requirements of the actor's practical interest . . . What I wish to emphasize is that the ideal of rationality is not and cannot be a *peculiar* feature of everyday thought nor can it therefore be a methodological principle of the interpretation of human sets in daily life."

Reconstructing the problem of rationality so as to hand it back to researchers consists in the proposal that sociologists cease treat-

ing the scientific rationalities as a methodological rule for interpreting human actions.

Procedurally speaking, how would an investigator act once he has ceased to treat the scientific rationalities as a methodological rule?

NORMS OF CONDUCT

When the above-mentioned rational properties of actions are conceived as norms of proper conduct, four meanings of such norms can be distinguished.

First, the norms may consist of the rationalities to which scientific observers subscribe as *ideal norms* of their activities as scientists. Second, the term may refer to rationalities as operative norms of actual scientific work. Empirically, the two sets of norms do not show point for point correspondence. For example, there is a routinization of problem design and solution as well as a trust of other investigators found in actual investigative operations which textbooks in methodology generally ignore. Third, the term may refer to the socially employed and socially sanctioned ideals of rationality. Here the reference is to those rationalities as standards of thought and conduct that remain in accord with a respect for the routine orders of action of everyday life. Such standards are referred to in everyday language as "reasonable" thinking and conduct. Fourth, there are the rationalities as operative norms of actual activities of daily life.

To use the rationalities as a methodological principle for the interpretation of human actions in daily life means to proceed as follows: (1) The ideal characteristics that scientific observers subscribe to as the ideal standards of their investigative and theorizing conduct are used to construct the model of a person who acts in a manner governed by these ideals. Von Neumann's game player, for example, is such a construction.[7]

[7] Consider his characteristics. He never overlooks a message; he extracts from a message all the information it bears; he names things properly and in proper time; he never forgets; he stores and recalls without distortion; he never acts on principle but only on the basis of an assessment of the consequences of a line of conduct for the problem of maximizing the chances of achieving the effect he seeks.

(2) After describing actual behaviors, one looks to the model for the discrepancies between the way in which a person so constructed would have acted and the way the actual person has acted. Questions like the following are then asked: Compared with the model, how much distortion is there in recall? What is the efficiency of the means that the actual person employed when they are viewed with reference to the observer's wider knowledge, this observer's wider knowledge being typified as "the current state of scientific information"? What constraints are there upon the use of norms of technical efficiency in the attainment of ends? How much and what kind of information is needed for decisions that are predicted on the consideration of all the scientifically relevant parameters of the problem, and how much of this information did the actual person have?

In a word, the model states the ways a person would act were he conceived to be acting as an ideal scientist. The question then follows: What accounts for the fact that actual persons do not match up, in fact rarely match up, even as scientists? In sum, the model of this rational man as a standard is used to furnish the basis of ironic comparison; and from this one gets the familiar distinctions between rational, non-rational, irrational, and a-rational conduct (Pareto, 1935; Levy, 1952).

But this model is merely one among an unlimited number that might be used. More importantly, *no necessity dictates its use.* To be sure, *a model* of rationality is necessary, but only for the task of deciding a definition of credible knowledge and then *only but unavoidably for scientific theorizing. It is not necessary and it is avoidable in theorizing activities employed in coming to terms with the affairs of everyday life.*

It is necessary for scientific theorizing but *not* because of any ontological characteristic of the events that scientists seek to conceive and describe. It is necessary because the rules that govern the use of their propositions as correct grounds for further inference, i.e., in the very definition of credible knowledge, describe such sanctionable procedures as, for example, not permitting two incompatible or

contradictory propositions both to be used as legitimate grounds for deducing the warrant of another proposition. Since the definition of credible knowledge, scientific or otherwise, consists of the rules that govern the use of propositions as grounds of further inference and action, the necessity of the model is provided by the decision in the first place to act in conformity with these rules (Kaufmann, 1944, pp. 48–66). The model of rationality for scientific theorizing literally consists of the theorizer's ideal that the meanings of these rules can be clearly explicated.

It is a consequence of the fact that actions of inquiry and interpretation are governed by what to common sense are the outlandish rules of scientific activities that the decision to use a proposition as grounds of further inference varies independently of whether or not the user can expect to be *socially* supported for using it. But in activities governed by the presuppositions of daily life the body of credible knowledge is not subject to such rigid restrictions regarding the use of propositions as legitimate grounds for further inference and action. Within the rules of relevance of everyday life, a correctly used proposition is one for whose use the user specifically expects to be socially supported.

RATIONALITIES AS DATA

No necessity dictates that a definition of rational action be decided in order to conceive a field of observable events of conduct. This result has the important and paradoxical consequence of permitting us to study the properties of rational action more closely than ever before.[8] Instead of using the vision of the ideal scientist as a means for constructing descriptive categories of behavior —and rational, non-rational, irrational, and a-rational are such categories—the rational characteristics of activities may be addressed with the empirical task of describing them as they are found separately in the above list of rationalities or in clusters of these characteristics. The uses, then, would look to the conditions of the actor's make-up and to his characteristic relationships to others as factors that might account for the presence of these rationalities, but without ironic comparison.

Instead of the properties of rationality being treated as a methodological principle for interpreting activity, they are treated only as empirically problematical material. They have the status only of data and have to be accounted for in the same way that the more familiar properties of conduct are accounted for. Just as we might ask how the properties of a status arrangement are relevant to the incidence of striving behavior, or organized dissent, or scapegoating, or to the chances of occupational mobility or whatever, so we might ask how the properties of a status arrangement are determinative of the extent to which the actions of the actors show the rationalities. Questions such as the following, then, press for answers: Why are rationalities of scientific theorizing disruptive of the continuities of action governed by the attitude of daily life? What is there about social arrangements that makes it impossible to transform the two "attitudes" into each other without severe disruptions of the continuous activity governed by each? What must social arrangements be like in order that large numbers of persons, as we know them in our society today, can not only adopt the scientific attitude with impunity but can for their success in employing it make substantial claims for a living upon those to whom the attitude is foreign and in many cases repugnant? In a word, the rational properties of conduct may be removed by sociologists from the domain of philosophical commentary and given over to empirical research.

It is possible to state a general rule which subsumes innumerable research problems: *Any factor that we take to be conditional of any of the properties of activities is a factor that is conditional of the rationalities.* This rule sets up the claim that such factors, for example, as territorial arrangements, the number of persons in a net, rates of turnover, rules governing who can communicate with whom, timing patterns of messages, the distribu-

[8] It is through the absence of the "scientific rationalities" in the actions that constitute the routine social structures that rational action becomes problematical in the ways intended in Max Weber's neglected distinction between formal and substantive rationality.

tions of information as well as the operations for altering these distributions, the number and location of information "transformation" points, the properties of coding rules and language, the stability, social routines, the structured or *ad hoc* incidence of strain in a system, the properties of prestige and power arrangements, and so on, are to be considered determinative of the rational properties of actions governed by the attitude of daily life.

The immediate need is for an instrument that can be used to estimate the clusterings of the rationalities and their extensiveness for the actions of persons found at strategically different places in the social structure.

CONCLUSIONS

It has been the purpose of this paper to recommend the hypothesis that the scientific rationalities can be employed only as ineffective ideals in the actions governed by the presuppositions of everyday life. The scientific rationalities are neither stable features nor sanctionable ideals of daily routines, and any attempt to stabilize these properties or to enforce conformity to them in the conduct of everyday affairs will magnify the senseless character of a person's behavioral environment and multiply the anomic features of the system of interaction.

Bold as the statement may be, it does no violence whatever; and, in fact, it is entirely compatible with current sociological discourse and current sociological theories and findings. Sociologists have long been concerned with the task of describing the conditions of organized social life under which the phenomena of rationality in conduct occur. One of these conditions is continually documented in sociological writings: routine as a necessary condition of rational action. Max Weber, in his neglected distinction between substantive rationality and formal rationality, and almost alone among sociological theorists, used the distinction between the two sets of rationalities throughout his work, although he proposed the distinction on the basis of a keen methodological intuition rather than on empirical grounds.

The relationships between routine and rationality are incongruous ones if they are viewed either according to everyday common sense or according to most philosophical teachings. But sociological inquiry accepts almost as a truism that the ability of a person to act "rationally"—that is, the ability of a person in *conducting his everyday affairs* to calculate, to project alternative plans of action, to select before the actual fall of events the conditions under which he will follow one plan or another, to give priority in a selection of means to their technical efficacy and the rest—depends upon the fact that the person must be able literally to take for granted, to take under trust, a vast array of features of the social order. In order to treat rationally the $\frac{1}{10}$th of his situation that, like an iceberg, appears above the water, he must be able to treat the $\frac{9}{10}$ths that lies below as an unquestioned and, even more interestingly, as an unquestionable background of matters that are demonstrably relevant to his calculation, but which appear without being noticed. In his famous discussion of the normative backgrounds of activity, Emile Durkheim made much of the point that the validity and understandability of the stated terms of a contract depended upon the unstated terms that the contracting parties took for granted as binding upon their action.

The sociologist refers to these trusted, taken for granted, background features of the person's situation, that is, the routine aspects of the situation that permit "rational action," as mores and folkways. Among sociologists the mores depict the ways in which routine is a condition for the appearance of rational action or, in psychiatric terms, for the operativeness of the "reality principle." The mores have been used, thereby, to show how the stability of social routine is a condition which enables persons in the course of their everyday affairs to recognize each other's actions, beliefs, aspirations, feelings, and the like as reasonable, normal, legitimate, understandable, and realistic.

REFERENCES

Kaufmann, Felix. *Methodology of the social sciences.* New York: Oxford Univ. Press, 1944.

Levy, M. J. Jr. *The structure of society*. Princeton, N. J.: Princeton Univ. Press, 1952.

Pareto, Vilfredo. *The mind and society*. Translated by Livingston, A. & Bongiorno, A. New York: Harcourt Brace, 1935.

Schutz, Alfred. The problem of rationality in the social world. *Economica*, 1943, 10, 130–149.

Schutz, Alfred. The stranger. *Am. J. Sociol.*, 1944, 49, 499–507.

Schutz, Alfred. On multiple realities. *Phil. Phenom. Res.*, 1945, 5(4), 533–575.

Schutz, Alfred. Choosing among projects of action. *Phil. Phenom. Res.*, 1951, 12, 161–184.

Schutz, Alfred. Common sense and scientific interpretation of human action. *Phil. Phenom. Res.*, 1953, 14, 1–38.

von Neumann, J. & Morgenstern, O. *Theory of games and economic behavior*. Princeton, N. J.: Princeton Univ. Press, 1947, p. 79.

(Manuscript received November 3, 1958)

"I don't know what you mean by 'glory,' " Alice said. Humpty Dumpty smiled contemptuously. "Of course you don't—till I tell you. I meant 'there's a nice knock-down argument for you'!"

"But 'glory' doesn't mean 'a nice knock-down argument,' " Alice objected.

"When *I* use a word," Humpty Dumpty said, in rather a scornful tone, "it means just what I choose it to mean—neither more nor less."

"The question is," said Alice, "whether you *can* make words mean so many different things."

"The question is," said Humpty Dumpty, "which is to be master—that's all."

LEWIS CARROLL, *Through the Looking-Glass*

BOOK REVIEWS

Richard L. Masland, Seymour B. Sarason, and Thomas Gladwin, *Mental subnormality. Biological, psychological and cultural factors.* New York: Basic Books, 1958. Pp. 442.

THIS survey of research by a neurologist, a psychologist, and an anthropologist was written at the request of the National Association for Retarded Children.

Masland presents a review of recent developments in human genetics as well as an account of some studies on environmental factors during early and later stages of pregnancy and in infancy, while Sarason and Gladwin present 'capita selecta' on the role of cultural factors in complicating the evaluation of psychological test performance.

Their review includes a brief presentation of Guilford's recent ideas on the structure of intelligence (in the normal or superior adult) and a plea for the use of the multifactorial concept of intelligence in studies of subnormal performance. This reviewer strongly supports this plea as well as the authors' proposal for comparative studies of the multifactorial structure of intelligence in groups at varying intellectual levels as well as of diverse cultural backgrounds.

In their jointly written introduction, the authors record explicitly their decision to write separate reviews of the two broad areas relating to the causation of mental subnormality, i.e., anatomical or biochemical abnormalities and cultural factors. It is regrettable that no effort was made to write a joint chapter containing recommendations for studies aimed at broadening our understanding of the interaction of both types of causative forces, such as investigations of the early environment and of the biochemistry in mongoloids of borderline intelligence or of infants with phenylketonuria while on a phenylalanine-free diet.

While this volume does not cover all the material one might expect in a text book on mental subnormality, it might have served as a refresher course for research workers in this field if the authors had thus cooperated. As it is, the review by Masland is too succinct to be useful for psychologists and sociologists. The review by Sarason and Gladwin may be more useful to the biologically trained scientist but is perhaps too weak in its systematic joints to be useful for a wider audience.

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Rollo May, Ernest Angel, and Henri F. Ellenberger, Editors. *Existence.* New York: Basic Books, 1958.

THIS book offers the first extensive introduction, in the English language, to the phenomenological and existential analytic schools of psychiatry in Europe. It is surprising that, even today, there exists such a wide difference between European and American psychiatry. Even more strange is the fact that on either side of the Atlantic one is largely ignorant of what goes on on the other side. The present book may well help to make us more aware of recent thinking in European psychiatry.

In the first three chapters the authors give a short introduction to the history, psychotherapeutic and theoretical implications of phenomenology and existential analysis. This is done in clear and readable English, unusual in this field where writing is so often pompous. The remaining 300 pages contain translations of papers that are representative of different variations and stages of the development of phenomenology and existential analysis. Most of these papers are well chosen. However their organization in the book could give the false impression that each paper represents a stage in a development that culminates in the existential analysis of Binswanger. In reality, the different approaches still exist side by side. An important omission is the absence of any of the work of Medard Boss, who pos-

sibly has gone a step beyond Binswanger. The inclusion of the article by R. Kuhn seems to add little but confusion to the understanding the reader may be able to obtain in the rest of the book.

The article by Henri F. Ellenberger is especially clear and instructive. This author gives an overview of the different approaches in psychiatry that are indicated by the name phenomenology:

1. Descriptive phenomenology, introduced by Karl Jaspers. This method relies mainly on verbal and nonverbal communications from the patient in order to obtain insight into his inner experiences.

2. The genetic-structural method (in the book represented by the papers of E. Minkowski and E. Straus). This approach starts out in a way similar to the descriptive method; but it tries to elicit a central disturbance (e.g., the subjective way of experiencing time), which presumably makes the total of the patient's disturbances understandable.

3. Categorical phenomenology. In this method "... the phenomenologist tries to reconstruct the inner world of his patients through an analysis of their manner of experiencing time, space, causality, materiality and other categories."

While the phenomenologist limits himself basically to the investigation of "states of consciousness," the existential analyst integrates such findings into an interpretation of the "mode of existence" of a patient. The existential philosophy of M. Heidegger serves as a frame of reference for this interpretation. The most extensive example of an existential analysis is the beautifully described case of Ellen West by L. Binswanger, especially translated for this book. A more theoretical discussion of this method is given in the paper "The existential analytic school of thought" also by L. Binswanger. The value of phenomenology and existential analysis is still a controversial issue, even in Europe. It is not likely that these methods will be met with much enthusiasm in the United States. But however this may be, the confrontation with these approaches may well be fruitful in this field where our understanding is still far from satisfactory. For this reason this book merits the attention of every psychiatrist or scientist interested in human behavior.

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Medard Boss, *The analysis of dreams.* New York: Philosophical Library, 1958.

MEDARD BOSS, Professor of Psychiatry at the University of Zürich, was originally trained in Freudian analysis. Confronted with the development of existential philosophy (especially by M. Heidegger) and of existential analysis (Binswanger) in Europe, he has formed a psychotherapeutic approach that is a compromise of Freudian technique and "existentialistic" interpretation. In the present book he shows what light this frame of reference can throw on the old problem of dream interpretation. In the first part of the book he discusses briefly the different dream theories that are current (those of S. Freud, C. G. Jung, H. Silberer, E. Fromm, and others). Each of the theories is subjected to specific criticism. The Freudian theory, for instance, is criticized for the ways in which the original dream thoughts are reconstructed from the manifest dream content (by reversing condensation, displacement, etc.) with which "Reconstructions of latent dream thoughts Freud has without doubt opened wide the doors to arbitrariness and violence of fact." In the remainder of the book, Medard Boss develops his own method of dream analysis. His approach is novel, stimulating, and most of the time clearly formulated and well illustrated by dream examples (the author claims to have 11,200 written records of dreams). In essence, Boss wants to let the dream "speak for itself." He takes the dream seriously, as it is presented. He lets the dream tell its own story, and then he asks the question: "What mode of existence did a dreamer have in the dream that such a dream became possible?" For instance, if in the dream there is an abundance of food

that seemed extremely attractive, we may conclude that it is quite likely that the dreamer at that time was in a state of hunger. The intentionality of the dreamer shows itself in the dream. The dream then is taken as seriously as waking life. While in waking life, the way in which we perceive our environment tells the story of our way of existing at that time, likewise, the dream will tell the same about the dreamer. Boss shows with a great many examples how he proceeds in this way to extricate meaningful material from the dreams. Boss reasons that the mode of the dreamer's existence is also a fundamental possibility for him during waking life. Dream analysis, therefore, can extend our understanding of the patient and our awareness of his potentialities.

Some aspects of the exposition are certainly open to criticism. For instance, it never becomes clear what is really the difference between waking and dreaming. Nevertheless, this book opens a new way of looking at dreams. The strongest point of this method lies in its simplicity; no intricate unprovable mechanisms of dream-thought transformation have to be presupposed in order to see the dream as a meaningful phenomenon.

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Abstraction expresses nature's mode of interaction and is not merely mental. When it abstracts, thought is merely conforming to nature—or rather, it is exhibiting itself as an element in nature. Synthesis and analysis require each other. Such a conception is paradoxical if you will persist in thinking of the actual world as a collection of passive actual substances with their private characters or qualities. In that case, it must be nonsense to ask, how one such substance can form a component in the make-up of another such substance. So long as this conception is retained, the difficulty is not relieved by calling each actual substance an event, or a pattern, or an occasion. The difficulty, which arises for such a conception, is to explain how the substances can be actually together in a sense derivative from that in which each individual substance is actual. But the conception of the world here adopted is that of functional activity. By that I mean that every actual thing is something by reason of its activity; whereby its nature consists in its relevance to other things, and its individuality consists in its synthesis of other things so far as they are relevant to it.

A. N. WHITEHEAD, *Symbolism*

COMPUTERS IN BEHAVIORAL SCIENCE

Logic By Machine: Programming the LGP-30 to Solve Problems in Symbolic Logic*, *Paul W. Hagensick, Ohio University*

The purpose of this paper is to describe a program enabling a digital computer to solve problems in symbolic logic. At present these problems are limited to expressions that can be formulated in propositional logic. With relatively minor modifications, it is believed that the range of problems solved by machine will be extended to include some of the functional calculi. The possible scope of this extension has not been fully explored as yet. Within propositional logic, the program that is being devised is capable of accepting any complex expression in which any number of propositional abbreviations are associated with the connecting terms 'and,' 'or,' 'if . . ., then,' 'if, and only if,' or 'not.' The only limitations in the program are: (1) the propositional expression must be formulated in a specified symbolic form (that happens to be reasonably close to that of Principia Mathematica); (2) the expression may not contain more component parts than the machine involved can, because of its design, accept. In the machine used to develop this program, this practical limit happens to be about 2000 characters. Within these limits, the logic program will do the following: (1) determine whether a given expression is logically true; (2) determine whether a given expression is logically false; (3) determine and print out the simplest form of the given logical expression if it is neither a contradiction nor a tautology. By the simplest form of a complex propositional statement I mean one in which all redundancy has been eliminated, all parentheses removed, all contradictory elements detected and eliminated, and all tautologies detected and eliminated. The basic problem in the logic program is to find how to teach a machine to do these things.

It has long been recognized that the logical import of a statement in propositional logic can be completely and conclusively determined by using truth-tables. By tabulating all possible combinations of true and false, a statement that is logically true will be true for every possible arrangement of truth-values among its constituent parts; a contradiction, or logically false statement, will always show any arrangement to be false; a statement not logically determinable will always show that at least one arrangement of its parts is true and at least one is false. The important point to note is that truth-tables can be used to test propositions with any degree of complexity whatever, provided only that they can be stated in propositional logic. With this in mind it was thought that the problem of how to program a machine to do logical problems could be solved by having the machine automatically generate what amounts to a truth-table of whatever degree of complexity was required. However, a serious difficulty arose almost immediately when this kind of program was written. While such a program can process propositional expressions containing a large number of units, it cannot process expressions containing more than 15 different propositions, on the machine used. Each new and different proposition doubles the previous number of entries in its truth-table. By the time 15 different propositions are introduced, the truth-table for the expression contains more than 32,000 entries. To add even one more new proposition would require over 64,000 entries in the table for the expression. This is beyond the physical limitations of the computer used.

The resolution of this problem proved, in principle though not in fact, to be surprisingly simple. All that we had to teach

* This paper is a revised and somewhat expanded edition of a paper read at the first annual meeting of the LGP-30 Users Pool in Washington, D. C., April 14, 1958. Omitted in reading the paper were some of the details of the design of this computer and some of the more technical features of the logic program.

the machine to do was to find what is termed the 'disjunctive normal form' of the complex proposition to be processed. Then, this result is negated and its disjunctive normal form is found (if the original expression was not a self-contradiction). By the disjunctive normal form of a given expression I mean an equivalent expression consisting of a group of conjunctions joined by inclusive 'or's.' The conventions adopted permit this normal form to be expressed using only letters of the alphabet to represent propositions, a dash preceding a letter to represent the negation of a proposition, and the wedge (**v**), the conventional representation of inclusive 'or.' Thus, the propositional form $A\ (\ B \vee C\ )$ when processed by the machine will read $AB \vee CA$. The principles on which the logic program is based are: (1) the disjunctive normal form of a contradiction will contain a series of disjuncts each of which contains a contradiction; (2) the denial of a tautology, or logically true statement, is a contradiction; and finally (3) it is always possible to find a disjunctive normal form equivalent to any expression in propositional logic. This includes not only single, complex statements but also arguments with premises and a conclusion. In this latter case the program will, in effect, determine whether arguments are valid or invalid. This will be accomplished by associating premises conjunctively and tying the group of premises to the conclusion with the 'if, . . . then . . .' relation—so called 'material implication.'

Using normal forms bypasses the physical limitation of truth-tables. It is now possible to check logical expressions containing hundreds of different propositions. Indeed, in a machine with an unlimited memory and with only minor modifications of such a logic program, it would be possible to process expressions containing over 2 billion different propositions.

Constructing a program to teach an ordinary digital computer to solve logical problems of any degree of complexity whatever (limited only by the physical characteristics of the machine used) would be virtually impossible without subdividing the project into relatively small parts, i.e., subroutines. In the final logic program there may be as many as 2000 different instructions. To write a single program with this number of instructions would be as frustrating as it would be unnecessary. We have, therefore, carefully dissected the processes involved in the logic program into a series of limited programs that can be checked independently of each other.

Subroutines

The subroutines that comprise the logic program are the following:

1. The initial coding. The logical expression is written initially in a close approximation of ordinary propositional logic. This includes suppression of the sign for conjunction (&) and suppression of parentheses around conjunctions. It is not possible for the programmed machine to process the expression in this form. The machine operates with binary arithmetic. It will, then, need to determine how to code a given logical expression so that it will be possible for the machine to distinguish between a binary number representing a propositional variable and a binary number representing one of the logical signs in the expression. It is also important at this time to establish a code that will permit the machine to make any change in the given expression that will be needed in order to establish the equivalent normal form. The code representation at this point will thus be determined by the purpose of the program and the nature of the machine involved.

The computer used in this study is called the LGP-30 and is manufactured by the Royal McBee Company. A distinctive feature of this machine is that the portion of the computer on which information is stored consists of a magnetic drum called its memory unit. This drum is divided into 64 sections, called 'tracks'; each track is divided into 64 sections called 'sectors'; each sector has 31 usable positions that can be either electrically charged or not. There are, on this machine, 4096 (64 x 64) sectors that can be set to contain a 31 position binary number. On any single operation, the only thing that the machine is capable of discriminating with respect to any one of the 4096 sectors is whether the first position (from the left) of

that sector is or is not electrically charged. Each position on a sector is called a 'bit'; the first position is called the 'sign bit.' In coding a logical expression, as we proceed from the first entry in the expression, every character that appears is coded on a separate, successively higher numbered sector. Four decimal numbers are used to identify locations in memory. The first 2 (from the left) specify the track, the next 2 the sector. Thus the address 1847 is the address of the sector 47 on track 18. If now we are coding the expression: $A(B \vee C)D.$ on, say, track 32, we place the binary representation of A at track 32 sector 00; we place the representation of the opening parenthesis at 3201; the representation of B at 3202; the representation of $\vee$ at 3203; C at 3204; closing parenthesis at 3205; D at 3206; and the representation of the period to indicate the end of the expression at 3207. The actual code employed at present uses only the first six bit positions on each sector—though this will be modified later. At this point, however, we have been content to code 16 alphabet letters to represent different propositions, their negations, the logical signs for $\vee$, &, $\rightarrow$, $\leftrightarrow$, and the signs for opening parenthesis, negated opening parenthesis, closing parenthesis and period. By simply using more bit positions to represent other propositions we can easily modify the program to accommodate an almost unlimited number of different propositions. The practical difficulty is that there are only 4096 sectors in the memory and almost half of these will be used in writing the logic program. Still, a statement in propositional logic containing, say, several hundred different propositions with a total of, say, 1500 different characters arranged in any possible way is hardly unimpressive. To be able to determine entirely by machine operations what can be said logically about the expression and its simplest form if it is neither a self-contradiction nor a tautology is, I believe, an achievement of some magnitude.

2. Ampersand addition. When the propositional expression was initially coded, it was noted that ampersands, for 'and,' were, by agreement, suppressed. It now appears that at one point in the logic program it will be convenient to have ampersands not suppressed, but, rather, contained in the expression being processed. This is the case when negated groups are being eliminated. We now have the machine go through the expression being processed and, wherever they have been suppressed, add ampersands. This task is somewhat more complicated than it seems at first glance. To add a single ampersand to an expression requires that this sign be assigned an appropriate address. But this means that the address of every character following the added ampersand must be changed to an address that is one greater. Thus, in the propositional function previously mentioned, the expression is located on the first through the eighth sector of track 32:

3200	A
01	(
02	B
03	$\vee$
04	C
05	)
06	D
07	.

When suppressed ampersands are added the expression will read:

3200	A
01	&
02	(
03	B
04	$\vee$
05	C
06	)
07	&
08	D
09	.

In the actual logic program, subroutines 1 and 2 are performed by the machine in a single set of operations. This is accomplished by programming the machine to add an ampersand after every character unless the character is a $\vee$, (, $-($, $\rightarrow$, $\leftrightarrow$, or . .

3. Arrow removal. It is useful to be able to include a sign standing for logical implication (If . . . , then . . .) in expressions to be processed by machine. The logic program is not capable of testing such expressions, however. As a consequence, a subroutine has been constructed that will eliminate the arrow or horseshoe standing for implication by replacing the given expression with an equivalent one not containing such a sign.

The arrow, in propositional logic, is conventionally defined as meaning that the 'if-clause' is false or the 'then-clause' is true. I.e.:

$$P \rightarrow Q = -P \vee Q$$

Using the definition as its cue, the machine is taught, upon reaching and identifying an arrow, to replace it with the wedge and replace its 'if-clause' with its negation. The program is designed to make a series of inspections of the given expression until all arrows have been eliminated.

4. Double-arrow removal. The double-arrow, standing for logical equivalence, is also a convenient sign to be able to process since it occurs with some frequency in propositional logic. This sign can be eliminated in a number of ways, using any of the following equivalences:

$$P \leftrightarrow Q = (-P \vee Q) \ \& \ (P \vee -Q)$$
$$P \leftrightarrow Q = (P \rightarrow Q) \ \& \ (Q \rightarrow P)$$
$$P \leftrightarrow Q = -(P \ \& \ -Q) \ \& \ -(Q \ \& \ -P)$$

It will be noted that in each of these equivalences the number of terms in the right hand expression is one more than double the number on the left. Thus, the expression substituted for a double-arrow must be expanded if it is to be equivalent. This is not a particularly pressing problem when the given expression happens to contain just one double-arrow. However, serious theoretical problems arise when either side of the double-arrow happens to be complex and happens to contain a double-arrow. Thus, the following cases are troublesome:

(1) $(A \leftrightarrow B) \leftrightarrow C$
(2) $A \leftrightarrow (B \leftrightarrow C)$
(3) $(A \leftrightarrow B) \leftrightarrow (C \leftrightarrow D)$

Because of the difficulties that arise in eliminating such cases, we have not as yet attempted to write the subroutine for double-arrow removal. We are, rather, planning to wait until further developments in the logic program, as indicated briefly near the close of this paper, are worked out.

5. Addition of parentheses around &'s inside of parentheses. During later subroutines it was determined that some of the parentheses that have been suppressed around &'s are not superfluous. Thus, for example, in the expression:

$$A \ \& \ B \vee C,$$

it is understood that A and B are both the case or C is the case. However, in the expression:

$$-(A \ \& \ B \vee C),$$

understood to be the denial of the previous expression, the suppression of the parentheses leads to a misinterpretation later in the program. The machine will process this by eliminating the negated opening parenthesis and, as explained below, will decide that the expression means:

$$(-A \vee -B \ \& \ -C)$$

But with this expression the machine would be unable to decide that there should be parentheses around the $\vee$ and would, in fact, proceed as though there were none. Consequently, the printed result will be:

$$-A \vee -B \ -C$$

The correct answer is:

$$-C \ -A \vee -B \ -C$$

It is, therefore, necessary to eliminate this source of machine error. This might be done in the negated parenthesis removal subroutine but would be inconvenient. And so, a special subroutine has been constructed to add parentheses around &'s that occur inside of negated parentheses. This happens to be one of the more difficult programs since various special cases that come up require separate, special treatment. The program has been written, however, but has not been fully debugged.

6. Negated parenthesis removal. This subroutine is of particular importance in the logic program. It is a program through which every expression must eventually go unless it is self-contradictory at the outset. Fortunately, it is not difficult in principle to eliminate negated parentheses since the terms being negated as a group are simply themselves denied, ampersands are changed to wedges, wedges are changed to ampersands, negated parentheses are unnegated, and unnegated parentheses are negated. The key to

the resolution of the problem by machine is to be able to take complements of coded expressions. The characters of an expression are coded in the first six bit positions on a sector. Each 6 bit character is a specified grouping of 1's and 0's from 000000 to 111111. The complement of each character is found by reversing all of its 1's and 0's. The characters representing propositions are so coded that the negation of the proposition is its complement. And, the code representation of the wedge is the complement of that representing the ampersand; the code representation of a negated parenthesis is the complement of an unnegated parenthesis. To find, by machine, the complement of any character, the character is simply subtracted from 6 ones in the first six bit positions. E.g., the code designation of A is:

001110

To find its complement, subtract it from 6 ones:

111111
001110 = A
110001 = $-A$

The code designation of $-($ is

101111

Its complement:

111111
−101111 = $-($
010000 = (

The practical difficulty in developing this program is similar to earlier problems that came up. The difficulty involves negated parentheses inside of negated parentheses. What is inside of such parentheses will not be complemented, though we must be prepared to resume complementing when we reach the closure of such parentheses. To do this we construct devices called complement-counters and paren-counters. The complement-counter is a storage sector that is set when the machine is to complement. Then, when a negated parenthesis inside a negated parenthesis is reached, the negated parenthesis is complemented but the complement-counter is made to read do not complement. At the same time a paren-counter is used to keep track of the closures of opening parentheses that are encountered.

7. Elimination of unnecessary parens and ampersands. During the program for eliminating negated parentheses it is convenient to have ampersands in the expressions since they are complemented and turned into wedges. Wedges have been replaced with ampersands. And it is now possible for there to be parentheses around terms conjoined with ampersands. These are, however, as noted earlier, by convention, unnecessary. Since there is no longer any need for ampersands and such unnecessary parentheses, they are eliminated in this section of the logic program. The machine removal of ampersands is accomplished by going through the expression one character at a time and moving it to a different section of the memory drum without moving the ampersands. Whenever a parenthesis is met a quick check is made to see whether it is necessary or not. This is equivalent to determining whether the units within the parentheses are conjoined with a wedge or an ampersand. When there is a wedge, the parentheses are necessary and are moved with the rest of the formula. When there is an ampersand, the parentheses are left behind.

8. Addition of necessary parentheses around v's. During the negated parenthesis removal program, there may occur more than two propositions conjoined with ampersands within a single set of parentheses. In that event, the program would produce an expression of the form:

$$(A \vee B \vee C)$$

This expression is not in the least objectionable since alternation is associative and communative and is, hence, not ambiguous in any grouping of its parts. However, later in the logic program, the parentheses remaining will be eliminated by applying a series of logical distributions. It would prove difficult and inconvenient to attempt to distribute an expression in which the same set of parentheses enclose more than one wedge. For this reason we will now go through the logical expression and determine whether there are any cases in which more than one wedge is

enclosed by a single set of parentheses. Parentheses will be added in such cases whether or not that portion of the expression is going to require distributing.

9. Preliminary search for contradictions. In order to test the accuracy of the actual logic program as operated by the machine, we need to check the answers given by solving some fairly complex problems by hand, outside of the machine. While performing such operations, it was noticed that a substantial number of operations could be eliminated whenever an expression of the following form turned up:

$$\ldots \vee A\ (B \vee C)\ -A \vee \ldots$$

That is, when propositional variables and their negations turned up between **v**'s but outside of any other parentheses, the whole expression between the **v**'s can be eliminated. It will, upon completion of processing, contain a series of contradictions anyway. During the subroutines that have been described to this point, one of the key machine operations has been to shift the given expression from one end of the machine to the other (exclusive of the tracks required for the program itself). In this way, characters can be eliminated by not being shifted and openings can be provided to add characters where necessary. As it happens, at this point, the first address of the given expression is the smallest possible location on the machine (this happens to be track 32 sector 00). In the distribution program that follows, it is convenient to shift forward, i.e., to make required shifts by moving toward the beginning of the expression. Consequently, the expression to be processed should be at the highest possible address to insure greatest possible space for shifting. For this reason the expression will be shifted from its present addresses to the other end of the machine. But in so shifting the expression, it will be inspected to see if contradictions occur outside of parentheses but inside of **v**'s that are not surrounded by parentheses. The machine will stop such expressions from being shifted and go to the next free **v**. This same subroutine may be used after each of a specified number of distributions since such contradictions may also emerge before all of the distributions are completed.

10. The distribution of expressions containing parentheses. 1st Part. The expression in the machine at this point contains propositional variables, **v**'s, and in some cases may contain parentheses around characters surrounding **v**'s. The expression will be in disjunctive normal form as soon as all such parentheses are eliminated. The machine has been programmed to use a well-known distribution law to eliminate the remaining parentheses by substitution of an equivalent expression:

For: $A(B \vee C)D$.
May be substituted: $DAB \vee CAD$.

In order to perform the distribution of an expression the machine needs to learn the location of the 1st address of the expression, the location of the addresses of the 1st opening parenthesis and its closure, and the location of the character indicating the scope of the expression to be distributed. Thus, in the following expression:

$$AB(C \vee D)\ E \vee AC\ (B \vee D).$$

the scope of the first distribution extends through the E preceding the wedge. The addresses of the first A, the first opening parenthesis, its closure and the wedge need to be determined in order to normalize the expression by distributing it. Upon locating these addresses, and performing the distribution, the expression will be:

$$EABC \vee DABE \vee AC\ (B \vee D).$$

In performing the distribution the address of the first character in the expression will be changed and its location stored in memory. Going through the expression a second time, the machine will come to a free **v** before encountering an opening parenthesis. It will now code $EABC$ in a second way to enable the processing to be completed. The first part of the distribution program determines whether distribution is required and if so this subroutine locates the four critical addresses that are needed.

11. Second part of distribution program. Distributing. Distributing logical expressions follows the pattern noted earlier. In the machine program it is convenient to consider

separately four possible patterns that may come up. These are:

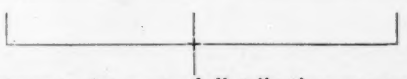
1. $(A \vee B)$.
2. $(B \vee C)\ A$.
3. $A(B \vee C)$.
4. $A(B \vee C)D$.

Characters may surround parentheses (Case 4); may appear only right of the closing parenthesis (Case 2); may appear only left of the opening parenthesis (Case 3); or may appear neither right nor left of the parentheses (Case 1). Case 1 does not require distribution, but does require elimination of parentheses before final processing. It is convenient to eliminate such parentheses at this point in the logic program. Cases 1 and 2 are easiest for the machine to process since they do not require space(s) to be provided 'inside of' the given expression. In both cases the addresses of the characters preceding the closing parenthesis are shifted by adding one to each address. This eliminates the closing parenthesis. Then, in case 1, 2 is added to the address of the first character in the formula and the machine returns to the first part of the distribution program to complete the processing. In case 2, the machine hold shifts[1] the contents of the addresses following the closing parenthesis and preceding the period (or next free wedge) to the address preceding the character following the opening parenthesis. Following this, the address of the first character is relocated. Thus:

Case 1	Case 2
1. $(A \vee B)$. Given	2. $(B \vee C)A$. Given
1.1 $((A \vee B$. Shift right 1 address	2.1 $((B \vee CA$. Shift right 1 address
1.2 $A \vee B$ Change 1st address	2.2 $(AB \vee CA$. Shift characters from (to in front of expression
	2.3 $AB \vee CA$ Change 1st address

In cases 3 and 4 the operations are somewhat more difficult since the interior of the expression must be opened to permit inclusion of the character(s) to the right of the opening parenthesis. In both cases, the addresses of the characters to the right of the opening parenthesis are incremented by one to eliminate the opening parenthesis. Then the whole expression, up to the closing parenthesis, is shifted to the left the number of characters to the right of the opening parenthesis minus one and the initial address is relocated. The characters preceding the opening parenthesis are hold shifted into the opening provided in front of and on top of the closing parenthesis. In case 3 the distribution is now complete and the machine returns to the 1st part of the distribution program. In case 4 the characters that originally followed the now defunct closing parenthesis are hold shifted in front of the first character and the machine returns to the first part of the distribution program after relocating the first address of the formula. Thus:

Case 3	Case 4
3. $AB\ (C \vee D)$. Given	4. $AB\ (C \vee D)E$. Given
3.1 $AABC \vee D)$. Shift AB one address right. Relocate 1st address	4.1 $AABC \vee D)E$. Shift AB one address right
3.2 $ABC \vee DD)$. Shift from A to D one address left. Relocate 1st address	4.2 $ABC \vee DD)E$. Shift A to D one address left
3.3 $ABC \vee DAB$. Place AB on (2nd) D).	4.3 $ABC \vee DABE$. Place AB on D).
	4.4 $EABC \vee DABE$. Shift E in front of A. Change initial address to that of shifted E



Return to 1st part of distribution program.

12. Second Coding Program. In the first part of the distribution program, the machine may encounter a period or free wedge before it meets an opening parenthesis. In this event, the expression preceding the free wedge or period consists of a single proposition or a group of propositions conjoined by (implicit) ampersands. In complicated distribution problems, a substantial number of conjunctions between free wedges

[1] In "hold shifting," the contents of the accumulator is placed in a specified address, but the accumulator remains unchanged.

may emerge. These are called "normalized expressions." A normalized expression may contain a number of propositions repeated more than once. All such duplication is superfluous and the repeated proposition can be eliminated. Furthermore, the expression may contain a proposition and its negation. In this case, the whole expression is false and can be eliminated. The second coding routine is designed to eliminate all repetitions among the propositions in a normalized expression. It will also eliminate self-contradictory normalized expressions. And, finally, it sets up the next phase of the logic program.

13. Simplification and Print Out Program. When the original expression has been entirely normalized and put through the second coding routine, whatever remains after eliminating redundancies and contradictions is simplified logically and the result printed out. Simplification is accomplished by eliminating normalized expressions that are identical to one that has already occurred and by eliminating normalized expressions that are contained in others.

We know that the original expression is not a self-contradiction if anything whatever is printed out. The original expression may be a tautology, however. In this event, the normal form of the denial of the expression printed out will be a self-contradiction. If this turns out to be the case, the first printed statement is the simplest form of the original and is a logical truth. Otherwise, the first printed statement is its simplest form but is not logically true.

This concludes the logic program. Although the actual program has been designed for use on the specific computer, the LGP-30, it is presumed that the program can be adapted for use on other machines. It may be difficult to so modify the actual program written for the LGP-30. It should not, however, be difficult for an experienced programmer to follow the steps outlined in this discussion in developing a logic program for other machines.

It is now recognized that there are major difficulties associated with the present form of the logic program. These difficulties are reasonably well understood although they have not been resolved. They involve inefficiency in programming and wasteful use of machine space. It is anticipated that future research will develop significant modifications of the present logic program. These will be reported later.

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IPL-V: The Newell-Shaw-Simon Programming Language*,

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Information processing language, version 5 (IPL-V), is the fifth in a series of programming languages developed by Newell, Shaw, and Simon (1957), and Newell and Shaw (1957) to simplify programming very complex processes on digital computers, and is an outgrowth of the work of these authors in the computer simulation of cognitive processes. The language is designed to be interpreted by a special computer program that effectively turns the machine being used into an IPL computer with a flexible memory structure and with the capacity for executing recursive subroutines. Earlier versions of the language could be used only with RAND's Johnniac computer, but recently interpretive systems have been coded for the IBM 650, 704, and 709 so that programs written in IPL-V can be executed on these machines.[1] In addition, the main features of IPL have been added to the FORTRAN language by Gerlernter, and have been the starting point for LISP, a list-processing language developed by McCarthy and his co-workers at the M.I.T. Computation Center.

Programming in IPL is relatively simple because a few instructions can accomplish a

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[1] Persons mainly responsible for coding the various interpreters are: 650, Nicholas N. Saber, University of Pittsburgh; 704, Edward Feigenbaum, Carnegie Institute of Technology, and George Mealy of Bell Telephone Laboratories; 709, Bert F. Green, Jr. and Alice K. Wolf, M.I.T. Lincoln Laboratory. In addition, Fred Tonge of RAND coded earlier versions of IPL and had a major role in designing IPL-V.

lot of processing. Also, the hierarchical nature of the language leads naturally to building the program out of small pieces, each of which is a short IPL routine to do a specific job. Combining the smaller units into larger routines is simple because very little must be done to insure that the various routines communicate properly with each other. Special push-down storage lists make communication remarkably simple. Finally, debugging is aided immensely by trace, snapshot, and post-mortem routines that print information in the symbols used by the programmer, wherever possible.

Properties of IPL lists

The IPL system processes information by manipulating lists of symbols. Each item of information in the computer is represented by either a symbol or a list. More precisely, each item is represented by a symbol that may be either an elementary unit, or may be the name of a list of symbols. Each symbol on a list may likewise name further lists, so a single item may be represented by a hierarchy of lists, called a list structure. A bridge deal, for example, might be represented as a main list with four sublists. Each sublist would be the list of cards in one player's hand, while the main list would consist of the names of the four sublists. The order of the four symbols on the main list could indicate the order of bidding and play, e.g., N, E, S, W. Interchanging E and W on the list would then correspond with East and West exchanging hands.

If the computer were programmed to play bridge, it would have to compute various characteristics of each hand, such as the number of cards in each suit, the Goren point count, the biddable suits, and perhaps others. To store these quantities, a special *attribute list* would be associated with the standard list. The attribute list would contain a pair of symbols for each attribute: a symbol denoting a particular attribute, e.g., Goren point count, followed by a symbol denoting the value of that attribute for the particular list being described, e.g., 14. In some cases the values of an attribute will be the name of a list of symbols, as in the case of the attribute "biddable suits." The list structure representing a bridge deal now has one main list, four sublists, each with an attribute list, each of which in turn has at least one sublist. IPL can operate with these data at any level of detail: its unit may be a symbol, a sublist, an attribute list, a main list, or the entire list structure.

Many kinds of non-numeric information are conveniently represented by list structures. Thus an expression in algebra of the form $((X + e)\,d)(ax^2 + bx + c)$ might be represented by a list structure with sublists for each level of complexity in connectives, as follows (the numbers indicate sublists, the symbols + and · are used for *plus* and *times*, and lists are shown horizontally—one symbol per cell). In Figure 1, the main list shows that the expression is equal to sub-expression 1 times sub-expression 2; sublist 2 shows that the second sub-expression is equal to sub-expression 4 plus sub-expression 5 + symbol c; etc.

A different use for lists is encountered in self-modifying programs. To return to the bridge example, the program might initially have a large number of possible rules to use in selecting an opening lead. As the program played games, its experience in opening leads might be represented by a list of the good rules for finding opening leads. The list might

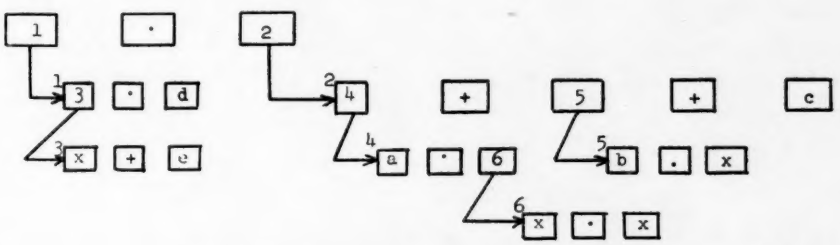

FIG. 1. List Structure for Algebraic Expression.

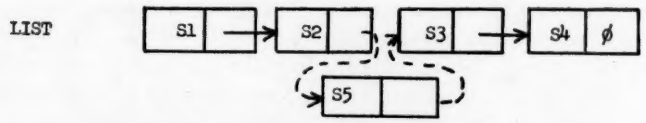

FIG. 2. Inserting a Symbol on a List.

Links are shown by arrows; the ϕ is a special list-termination symbol. S5 is inserted by changing the links as shown by the dotted lines.

indicate priorities, with rules being moved up on the list when they are successful and being demoted when they lead to disaster. Of course, a very sophisticated machine would keep several priority lists of opening leads, with descriptions of the situations in which to use each.

A major feature of IPL lists is that items can be added or deleted at any place on the list without disturbing anything else in the computer. The potential length of each list need not be specified in advance. Additions are made by using any available cell in the pool of cells not used by any other part of the system. The new cell is linked into the list by means of its address. Each cell contains *two* items of information—an IPL symbol and the location of the cell containing the next symbol on the list. Changing the latter item, called the LINK, alters the position of cells on a list. A new symbol is inserted in a list by putting the symbol in an available cell, linking that cell to the cell it is to precede on the list, and changing the link of the cell it is to follow, as shown in Figure 2. Variable-length lists are useful in the bridge example, because the play of the game involves selecting and removing one card from each hand on each trick. When we contemplate programming a game like Canasta, where list lengths would be continually changing, the advantages of variable-length lists become even more apparent.

IPL processes and routines

The IPL system includes over 100 built-in processes for manipulating symbols, lists, and list structures. Some examples of functions performed by these processes are: copy list *a*, erase list structure *a*, add symbol *a* at end of list *b*, test if symbol *a* matches symbol *b*, set *c* as the value of attribute *a* of list *b*, count the occurrences of symbol *a* on list *b*. In addition there are some input and output processes, and some special processes for dealing with numerical data. Most of the processes are *primitives*—subroutines written in machine language. The remainder are short IPL routines which combine several primitives. The machine language primitives, together with the interpreter, form the link between the language of the IPL programs and the language of the computer. When a primitive is to be executed, control is transferred from the interpreter to the primitive, and the machine language takes over. When the primitive has been completed, control is automatically transferred back to the interpreter.

An IPL routine is a series of IPL instructions for using various combinations of the built-in processes. There are only eight IPL instructions, six of which are used for setting up the inputs to the processes and storing the results of the process. The seventh is a conditional branch instruction, depending on the outcome of a process, and the eighth is the all-inclusive "execute" instruction, the argument of which is the name of the process to be executed. IPL permits two stages of indirectness in the specification of the locations involved in an instruction. Thus a symbol may be stored directly in the specified cell, or indirectly in the cell named in the specified cell, or doubly indirectly in the cell named in the cell named in the specified cell. Indirectness is especially useful in IPL. For example, the name of the list to be processed is often unknown when the program is written, since the name is the result of some previous processing, and the programmer knows only where the resulting name will be stored.

The all-powerful "execute" instruction is used not only to trigger the built-in processes but also to execute other IPL routines. In fact, IPL routines are generally built up out of combinations of other IPL routines, and

the entire program may well involve ten or more levels of routines, each routine at a lower level being executed by a routine at a higher level. By following special conventions, every routine may be written as a unit so that it will operate correctly no matter where it is executed in the hierarchy and no matter how many lower level routines it executes. To keep track of the hierarchy of subroutine references and the intermediate results of current processes, IPL uses devices called *push-down* storage cells, which are special lists used by all processes. Normally only the first symbol on a push-down list is directly accessible. Storing a symbol in a push-down list means adding the symbol at the head of the list, thereby pushing each symbol on the list down a notch, in the manner of a well for stacking plates in a cafeteria. Conversely, obtaining a symbol from a push-down cell means removing it from the head of the list, thereby popping up all the symbols beneath it. The inputs and outputs of all routines are made in a *communication* push-down list, and all the results of intermediate processing are kept in *working storage* push-down lists. By convention, each routine ends by restoring the *working storage* lists to the condition in which it found them, and also insures that all its inputs have been removed from the communication cell and its output placed there. At the same time, the IPL system uses another push-down list to keep track of the location of the next instruction to be performed in the routine that called for the present routine. This scheme insures that a hierarchy of routines, each calling on the next, can be executed without getting the inputs and outputs mixed up, and without losing one's place in the program, yet without taking special precautions in programming.

Recursive routines. The main advantage of push-down lists is not their convenience, but the fact that they enable routines to be recursive, i.e., to execute themselves as subroutines. In normal computer practice, subroutines that refer to themselves form endless loops, since each entry to the subroutine destroys the stored information about the location of the previous entry point, as well as possibly destroying the previous inputs. In IPL, a subroutine can use itself as often as it wishes; all intermediate data and all entry points will be pushed down on the lists, and assuming that an end point is finally reached, the whole sequence will be unravelled properly.

Recursive routines are the natural consequence of storing data in the form of list structures. Consider the problem of counting all occurrences of a given symbol in a given list structure. Starting with the main list, the plan will be to examine each symbol, in turn, on the list, and if the symbol matches the given symbol, to tally one; but if the symbol names a sublist, to execute the present program on the sublist. As another example, consider evaluating the algebraic expression given above, for specific values of the literal quantities. The program would examine the items on the main list, and would combine elements by the arithmetic process indicated by the connective, unless the elements named sublists, in which case the program would first execute itself on each sublist. Such a program would not care how many levels of sublists it found, i.e., how many recursions were necessary, provided that it eventually reached a level at which there were no more sublists. Consequently, a recursive program is very general.

The computer representation of IPL. How is an IPL program loaded and run on a computer? First, the IPL interpretive system must be loaded into the computer, in whatever is the standard manner for the computer in question, e.g., a binary tape is read into the 709. At this point, the computer contains the IPL loader, interpreter, and monitor, as well as the built-in processes. These programs comprise the IPL system and occupy from 3000–6000 registers in computer storage. The rest of storage is "available space." When the system has been put into the computer, the IPL loader starts reading in instructions or data written in IPL and translating them into digitally-coded words which are then stored in available space. When the program has been loaded, the loader transfers control to the interpreter, which proceeds to execute the program. Starting at the instruction indi-

cated by the programmer, it gets the instruction from storage, decodes it, and executes it. Each instruction has a LINK to the next instruction, so the interpreter can proceed down the list of instructions in the routine. For debugging purposes, the interpreter can be monitored, either continuously or selectively. A *trace* can be produced, showing each instruction that was performed, together with the current symbol on the top of the communication push-down list. Also, at programmed points, the entire contents of the communication and working storage push-down lists can be examined by a *snapshot* routine. Finally, the program can be stopped and a *post-mortem* taken, which shows all lists in the computer.

Uses of IPL

The accomplishments of IPL to date are limited to the work of Newell, Shaw, Simon and their collaborators. They first programmed the logic theorist—an IPL routine that could prove theorems in logical calculus. They have since elaborated the program so that it simulates in very great detail the way in which novices prove logic theorems. This work was aided by the construction of a General Problem Solver, a large IPL routine that forms the framework for a program to solve a large variety of problems. Current work includes a chess-playing program, further simulation of human problem solving behavior, and further work with the general problem solver. E. Feigenbaum is working on a model of elementary perceiving and memorizing (EPAM) and J. Feldman has programmed a model of human performance in binary choice experiments.

With IPL now generally available to behavioral scientists, we may expect an increase in its use to construct models of behavior. The successes of the RAND and Carnegie groups indicate that simulation experiments can be pursued very fruitfully. We can also be certain that new, unexpected ones for IPL will develop.

REFERENCES

Newell, A., & Shaw, J. C. Programming the logic theory machine. *Proceedings of the Western Joint Computer Conference*, IRE, February 1957.

Newell, A., Shaw, J. C., & Simon, H. A. Empirical explorations of the logic theory machines. *Proceedings of the Western Joint Computer Conference*, IRE, February 1957.

(Manuscript received May, 1959)

People, Places, Programs

A new Machine Computation Committee of the American Institute of Chemical Engineers (AIChE) sponsors the publication, in *Chemical Engineering Progress*, of abstracts of computer programs for mathematical techniques of interest to the chemical process industries. These include statistical programs dealing with regression analysis, non-linear estimation, curve fitting, curve plotting and response surface evaluation. In submitting programs, the author commits himself to prepare (for publication by the AIChE if of sufficient interest) a manual which includes a full description of the calculation method used, a logic diagram, and the detailed program itself for the particular computer involved. This manual is reviewed in the same way that literature articles are reviewed. In addition, the committee has the program run on the machine for which it has been prepared, to obtain an independent check-out.

The AIChE Machine Computation Committee has agreed to exchange information with *Computers in Behavioral Science*. Free copies of a 13 page booklet describing the procedure for submitting abstracts and manuals as well as for obtaining published manuals may be obtained from the AIChE, 25 W. 45th Street, New York 36, New York. Readers interested in reviewing programs should write to: Mr. J. W. Kellett, Design Engineering Division, Esso Research and Engineering Co., P. O. Box 209, Madison, New Jersey, specifying in detail the type of computer(s) available and the types of programs they would be interested in reviewing.

Biometrische Zeitschrift is a new quarterly for biometrics and biomathematics published by the Akademie-Verlag, Mohrenstr. 39, Berlin W 8. Each issue will contain 72 pages and the subscription price is D.M.

36. (approx. $9.00). The journal is edited by *Prof. Ottokar Heinisch* (Leipzig) and *Prof. Maria Pia Geppert* (Bad Nauheim) and is also the official journal of the German section of the Biometric Society.

IBM announces the Model 4 Type 650 console unit featuring 4000 words of drum storage for $800 a month additional rent over the present 2000 word storage. Field conversion to Model 4 is possible. The new machine is completely compatible with the old one.

ACT 1 is a compiler which translates from an algebraic language to the LGP-30 machine language. Copies can be obtained from Royal McBee Corp., Data Processing Division, Port Chester, N. Y.

A list of statistical programs available for the BENDIX G-15 D computer can be obtained from Bendix Computer Corp., 5630 Arbor Vitae St., Los Angeles 45, Calif., or from local representatives.

Dr. Raoul J. Freeman, of General Analysis Corporation, is developing a mathematical model which will lead to real estate assessment by an electronic computer program. Factors such as location, age, and type of property are among the many variables considered in arriving at property valuations. The advantages of the system are objectivity, economy, accuracy, speed, and documentation. Articles by Dr. Freeman on this subject can be found in the *Appraisal Journal* (April 1959), the *Michigan Municipal Review* (June 1959), and the *Journal of Land Economics* (November 1959).

COMPUTER PROGRAM ABSTRACTS

Matrix Rearrangement Program for IBM 650, *Donald Lamphiear and Steven G. Vandenberg, The University of Michigan* **(CPA 8)**

This program deletes any number of rows and columns and rearranges rows and columns of rectangular matrices (including square or symmetric matrices) of up to 1685 cells under the control of one or more parameter cards per matrix. The largest dimension of the original matrix cannot exceed 100. The program is written for University Board II **(CPA 7)**

A more detailed form of this paper has been deposited as Document number 6109 with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. A copy may be secured by citing the document number and by remitting $1.25 for photoprints, or $1.25 for 35mm microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

Varimax and Quartimax Rotations on the IBM 650, *Steven G. Vandenberg, The University of Michigan, and Donald Lamphiear, Bendix Aviation Corp.* **(CPA 9)**

A matrix of factor loadings is rotated (within tolerance limits specified by control card) into orthogonal axes which satisfy one of the following criteria:

a. Varimax criterion for normalized loadings
b. Varimax criterion for original loadings
c. Quartimax criterion for normalized loadings
d. Quartimax criterion for original loadings

(Normalization in (a) and (c) is across loadings for each variable to give all variables equal weight regardless of their communalities. After rotation the loadings are denormalized.)

This program requires index accumulators.

The transformation matrix is also punched.

The original matrix cannot exceed 953 elements. The number of elements plus the number of variables cannot exceed 999 elements.

Repetition with decreasing tolerance limits is optional.

Use with University Board II **(CPA 7)**.

A more detailed form of this paper has been deposited as Document number 6108, with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. A copy may be secured by citing the document number and by remitting $1.25 for photoprints, or $1.25 for 35mm microfilm. Advance payment is required. In addition, a SOAP list of the program may be obtained by ordering document number 6107 and remitting $3.75 for photoprints, or $2.00 for 35mm microfilm. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

IBM 650 Program to Arrange Factor Rotation Output in Tabular Form, *Kathleen Goode and Steven G. Vandenberg, The University of Michigan* **(CPA 10)**

The output from Varimax, Quartimax or Oblimax rotation is punched out complete with labels ready for listing on the IBM 407 in tabular format.

Use with University Board II **(CPA 7)**.

A more detailed form of this paper has been deposited as Document number 6129 with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. A copy may be secured by citing the document number and by remitting $1.25 for photoprints, or $1.25 for 35mm microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

A SWAC Regression Program Which Checks the Linearity of Regression by Epsilon, *Edward Levonian, Univ. of Calif., Los Angeles* **CPA 11**

For up to 64 variables for 412 data points, this program computes the regressions of each variable on the independent variable by calculating

$$\epsilon^2 = 1 - \frac{\left[\Sigma y^2 - \sum^{k} \frac{(\Sigma y_c)^2}{n_c}\right](N-1)}{\left[\Sigma y^2 - \frac{(\Sigma y)^2}{N}\right](N-k)},$$

where the independent variable is divided in $k \leqq 16$ categories, with n_c data points in category C. It also computes r. The program may be used for the variance ratio test of statistical significance in one way analysis of variance including the degenerate case of $k = 2$. A complete description of the program may be obtained from the American Documentation Institute by ordering Document number 6111 from the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. after advance payment of $1.25 for photoprints, or $1.25 for 35 mm microfilm. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

Statistical Extensions for Runcible I for the Basic IBM 650, *Staff, Computer Center and Statistical Laboratory, Case Institute of Technology.* **CPA 12**

This program will compute mean, covariance, alpha 3, alpha 4, correlation coefficient, size, standard deviation, highest and lowest value, linear equation, multiple regression, correlation matrix, gamma function, fixed point and floating point random number, cumulative normal probability, inverse cumulative normal and Poisson probability, cumulative Poisson probability, binomial and cumulative binomial probability, binomial coefficients, Chi square, t test, F test, simultaneous linear equation solver and matrix inverter. A copy of the detailed description may be obtained from the Computer Center.

"QUAP-4", Questionnaire Analysis Program I for the IBM 709, *John R. B. Whittlesey, UCLA Dept. of Psychiatry and Western Data Processing Center. Los Angeles, Calif.* **CPA 13**

This program tabulates and summarizes the results of questionnaires and surveys of the multiple-choice answer variety. Each questionnaire may have from 66 questions each with 100 possible choices up to 999 questions each with five or less choices. The questionnaire may be filled out by as many as 100,000 participants. Classification groups may be established and the results for the various classifications may be prepared separately, or as a composite group. Output is from magnetic tape onto the IBM-717 printer. There is a potential of 36 possible output formats, including formats for percentages of responses at each level, frequency counts, partial frequency counts, means and standard deviations, the Kolmogorov-Smirnov and Chi Square tests between pairs of classification groups, and other standard statistical measures. Header cards are used to control such factors as the number of questions, number of possible answers to each question, the arrangement of listings, and of comparison groupings, etc. Choice of analyses is made by putting a punch in the appropriate columns of the Header Card for each of the output formats desired. A detailed description of the program may be obtained by writing to the author.

Minimal IBM 650 Program for the Oblimax Rotation to Simple Structure, *Mary R. Nickles and Thomas A. Keenan, University of Rochester.* **CPA 14**

The Pinska and Saunders' solution for rotation to oblique simple structure is based on maximizing

$$K = \frac{\Sigma a_f^4}{(\Sigma a_f^2)^2},$$

where a_f is a factor loading and the summations are made over the entire matrix. This results in maximization of the skewness of the distribution of factor loadings, so that the fewest possible loadings account for the major part of the variance for each factor. The following restrictions hold

$$N \leqq 10 \quad n \leqq 100$$
$$0.0000 \leqq a_f \leqq 1.0000$$

where N is the number of factors and n, which must be *even*, is the number of tests. To estimate time required one may use the following rule of thumb: 1¼ seconds per rotation per variable. Input and output under control of University Standard Board II (see **CPA 7**). A detailed description of the program, as well as a punched card deck, may be obtained by writing to the Computing Center of the University of Rochester, Rochester, N. Y.

An Intercorrelation Program for the Datatron 202, *Robert C. Nichols, Dept. of Psychology, Purdue University.* **CPA 15**

The program computes means, standard deviations and intercorrelations for up to 80 variables for an unlimited set of observations. Input can be either on cards or tape. There are three methods of arranging data on cards, two of which allow more than 8 words per card for some data. Special equipment required: Automatic floating point and card input. Approximate time estimate: $2(N + 7)(.5n + .015n^2)$ seconds where N is the number of observations and n is the number of variables.

A detailed description and the program may be obtained from the author or by writing to the Purdue University Computing Laboratory for General Routine R-19.

An IBM 650 Program for the Tetrachoric Correlation, *Marilyn Charap and H. W. Garrison, Educational Testing Service.* **CPA 16**

This program will compute tetrachoric correlations between a maximum of 100 variables for up to 9999 cases. There can be no missing data. The input consists of the plus responses to all variables and all pairs of variables. The accuracy of the calculations depends on the number of iterations. Typical running times have been 10 by 10 matrix, 4 minutes; 41 by 41 matrix, 33 minutes; 50 by 50 matrix, 48 minutes. The program requires no special equipment. The computational method was developed by L. R. Tucker.

A complete listing of the program, wiring diagram, operating notes and details of the computational method have been deposited with the American Documentation Institute as Document number 6130. Upon advance payment of $3.75 for photoprints or $2.00 for 35 mm microfilm, copies may be obtained from the ADI Auxiliary Publication Project, Photoduplication Service, Library of Congress, Washington 25, D. C. Make checks payable to: Chief, Photoduplication Service, Library of Congress.

A Chi Square Program for the IBM 650, *Bernard A. Galler, The University of Michigan* **(CPA 17)**

This program accepts raw data from punched cards, accumulates cell frequencies and computes chi-squares for two variables at a time.

The sample size cannot exceed 999999 while M, the number of categories in any variable cannot exceed 9. A final restriction is $M^2K\ (2v - k - 1) \leq 2700$, where k is the number of dependent variables and v is the total number of variables. Punch out of the cell frequencies is optional. Use with University Board II **(CPA 7)**.

A more detailed form of this paper has been deposited as Document number 6110 with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. A copy may be secured by citing the document number and by remitting $1.25 for photoprints, or $1.25 for 35mm microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

F0031, A Program for the IBM 704 to Fit an Exponential Curve, *Paul A. Gillis, Westinghouse, Bettis Plant.* **(CPA 18)**

This program fits a set of up to 500 points to the curve

$$y = Ae^{Bx} + C$$

by the least squares technique. The program is compatible with COFIT.

A detailed description may be obtained from the American Documentation Institute Library of Congress, Washington 25 D. C. Order document 5926, enclosing $1.25 for a photoprint or $1.25 for a microfilm.

If science wins, the world will prove to be one in which man is thrown entirely on his own resources, skill, and self-control, his courage and his strength, perhaps on his ability to be happy in adjusting himself to pitiless fact. If science fails, there is room for childlike hopes that unseen powers may come to the aid of human weakness. If science wins, the world is the necessary consequence of logically related facts, and man's enterprise the playing of a game of chess against an opponent who never errs and never overlooks our errors. If science fails, the world resembles fairyland, and man's enterprise no longer a test of skill and knowledge, but conditioned by the goodness of his will or the possibility of luck.

WALTER T. MARVIN, Quoted from JOSEPH NEEDHAM'S *Time—the Refreshing River*

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

KOCHEN, M. AND GALANTER, E. H. **The acquisition and utilization of information in problem solving and thinking.** *Information and Control*, 1958, 1, 267–288.

The authors propose certain machine programs appropriate for finding regularities in binary series of events which they postulate to be isomorphic with the strategies (or programs) actually used by human subjects in finding such regularities. An example of a binary series would be 010010100101 $\cdots$. Two programs, one relying on extensive memory and test functions, the other on simple associations and similarities are proposed. The authors present data comparing the observed and calculated number of trials needed to discover regularities in periodic sequences. (*JWG*)

GREENBERG, H. **An analysis of traffic flow.** *Oper. Res.*, 1959, 7, 79–85.

The behavior of a stream of cars in traffic may be likened to the flow of a continuous fluid. The equations of fluid dynamics may then be used to describe the traffic velocity as a function of vehicle density. The solution of the partial differential equations of classical fluid dynamics yields the following equation for traffic velocity v in miles per hour as a function of traffic density k in vehicles per mile.

$$v = c \ln(k_j/k) \qquad (1)$$

where c is a constant and k_j is the vehicle density for a traffic jam ($v = 0$).

Alternatively it is possible to solve for the traffic flow in vehicles per hour, q, which is given by

$$q = c\, k \ln(k_j/k) \qquad (2)$$

This function will have a maximum value for $k_j/k = e$ which gives the optimum speed when $u = c$. This optimum speed depends on the constant, c, which must be determined for each roadway.

The equation (1) has been fitted to data gathered in the Lincoln Tunnel and on the Merritt Parkway. The least-squares fits give

$c = 228$ and $k_j = 17.2$ for the Tunnel

and

$c = 215$ and $k_j = 16.1$ for the Parkway.

The fit of the data to equation (1) is extremely good over the range of densities from 20 to 200 vehicles per mile that occurred during the test periods.

The fluid dynamic analogy to traffic flow is valid as long as there is a continuous wave interaction between vehicles. This occurs whenever a driver's speed and headway are controlled to a major extent by the other vehicles on the roadway.

This analogy may also be used to study the wave motion of the traffic stream, although no applications are made to this in the present paper. (*WJH*)

BRAINE, M. D. S. **The ontogeny of certain logical operations: Piaget's formulation examined by nonverbal methods.** *Psychol. Monogr.*, 1959, 73, No. 5, 1–43.

This article concerns itself with Piaget's theory and research of intelligence; it discusses some of the criticisms which can be leveled against this research; and it reports on studies designed to avoid such criticism which were undertaken by the author. The main features of Piaget's theory of intelligence are reviewed: i.e., his conception that the intellectual processes are isomorphic with the operations of bivalent logic, and his central postulate that "when the definitions of logical operations are interdependent, the intellectual processes isomorphic to these operations develop in association" (e.g., there should be a genetic association between the processes of order discrimination and the inferential response

$A > B.B > C. \supset .A > C$). The author also discusses Piaget's meaning of the terms conjunction and disjunction.

Piaget's views of the development of length measurement and of concepts of order are singled out for study by the author. A critical survey of these studies is given. It is pointed out that Piaget's techniques involve fairly complicated instructions to the subjects and hence require mastery of vocabulary over and above the mastery of the logical rules per se. Furthermore, in regard to the studies on length measurement, it is pointed out that Piaget's techniques require measurement skills which again are independent of mastery of the logical rules involved. The logical rule in regard to length measurement which the author concentrates on is the transitivity rule $A > B.B > C. \supset .A > C$. By nonverbal means of reinforcement, the subject was first taught the concept of "longer than," respectively "shorter than." In phases 2 and 3 of the experiment, it was then ascertained whether this relation was transitive or not. By means of a clever experimental design, it was possible to study correctness of response resulting from perception *vs* from inference by logical rules and also to control for memory of the instructions taught during phase 1. The findings indicated that the ability to make the logical inference $A > B.B > C. \supset .A > C$ is definitely related to age. Two tendencies emerged: one to apply the inferential rule; the other an attitude toward small differences. The author speculates (and presents some findings to support such speculation) that the latter attitude may depend on the former operation rather than vice versa. In regard to the studies on the ordering operation, the author discusses three relevant parameters: (1) the formal arrangement of the stimulus (linear, cyclic, etc.), (2) the number of objects to be ordered, and (3) the nature of the material ordered. The author points out that the latter two dimensions (not controlled by Piaget) have little to do with mastery of the order relation per se. The author shows that under the age of 4 years-2 months the child has no concept of order. However, it is shown that the number of elements to be ordered has an effect on success of the operation with age, thus leading Piaget to formulate emergence of the operation in children later than in effect warranted by the 'cleaner' data obtained by the author. Finally, the experiments allow the author to state that Piaget's basic postulate regarding the development-in-association of intellectual processes when the definitions of the isomorphic logical operations are interdependent is borne out by his data. A final section of the article contains a discussion of the relevance to Piaget's theory of the findings from longitudinal studies of the development of intelligence as measured by intelligence tests. (*JWG*)

GRIMSDALE, R. L., SUMNER, F. H., TUNIS, C. J. and KILBURN, T. **A system for the automatic recognition of patterns.** *Proceedings of the Institution of Electrical Engineers*, 1959, 106 (Part B), 210–221.

This paper describes a method for the automatic recognition of spatial patterns that is designed to be flexible enough to identify, and thus to "perceive," any form. A detailed description of the recognition logic is given, along with illustrations and examples of the discriminations made by the electronic computer (Manchester Mark I) that has been programmed for this work. The results presented treat the recognition of the subset of forms encountered in printed text, but without any limitations as to type font, upper or lower case, noise in the reproduction, rotation, position, or size. Examples pictured in the text include correctly identified representations of all of the above; in many instances the difficulty of identification, due to noise or distortion, appears great enough to give the human trouble.

The pattern to be recognized is first presented to a flying spot scanner that reduces the picture to a 40 x 64 black-white raster. Computer scanning then identifies "groups" of connect black cell "pattern points," by means of subroutines that identify neighboring black points and overlaps between matrix rows. Noise and gap counts are computed for the entire matrix, and these computations are used to adjust the pro-

gram's tolerance for small discontinuities. Edges are smoothed by means of averaging subroutines. Other subroutines compute lengths and angles of rotation, and reduce thick lines to thin. Tests of edge continuity and of curvature (differences and differences of differences are computed between edge points in successive rows) are made, and a group is assessed as completed when certain pre-set limits are exceeded. Successive groups are processed and characterized in this manner, until the entire figure has been completed. An assembly program now recombines these groups by means of matching sub-elements according to curvature and to slope, so that divisions between groups now fall at points such as those where curves change into straight lines, or lines meet or cross. Finally, a table of joins between these groups, in which, for each group, a list of the position (right, middle or left), and direction (top or bottom) of each join is made. A search of the tables of patterns of previously analyzed forms is now made. When an unambiguous identification is achieved, the answer is printed out. When several different forms meet the tolerance requirements of the computer program, they are all printed, along with their angles of rotations and estimates of similarity. When no matching can be made, the form is identified as unknown. If the experimenter wishes to "name" it for the computer, it can now be added to the computer's list of forms. The original list is in fact made in this manner, the computer doing all of its own processing of the original standard array of characters to be identified.

"A large range of patterns" is successfully identified by the computer on the basis of a library of standard patterns. Average time to identification is 60 seconds on the Mark I. It appears that the present system can be used to identify handwriting, with the expansion of the size of the identification table (which at present can handle only eight elements). (*LU*)

RUSSELL, R. W. **Brain, memory, learning: A neurologist's view.** Oxford: The Clarendon Press, 1959, Pp. XII + 140, 18*s*.

In this brief exposition, the author applies his clinical experience with many kinds of head injuries to problems in the physiology of learning and memory. Concussions, temporal lobe seizures, aphasias, and Korsakoff's syndrome are among the clinical entities used to derive theories of the functions of hippocampus and hypothalamic-frontal lobe system. Further information relative to frontal lobe activities comes from surgical and accidental trauma in this region. A comparison of such trauma occurring in adults with similar ones occurring in childhood suggests to the author a primary and very early role of the frontal lobes in "setting" emotional control of learning and of other behavior patterns. A chapter on the traumatic amnesias supplies material on the relative vulnerabilities of old and new memories. Some other subjects briefly discussed include: epilepsy, consciousness, pain, and the problem of attention. (*EM*)

LUCE, R. D. **On the possible psychophysical laws.** *Psychol. Rev.*, 1959, 66, 81–95.

The author shows that the type of scale on which a pair of variables are measured imposes restrictions on the type of mathematical law which can connect the variables. A scale type is defined by the group of transformations which leave the associated measurement theory invariant. For example, if the only measurement which can be applied to a set of objects is a simple ordering, and if a number is assigned to each object, then any monotone transformation on the set of numbers will preserve the order relation. If, however, measurement is done on an interval scale (as in temperature), then only linear transformations will preserve the essential feature, namely, the ratios of differences.

Examining the groups of admissible transformations for each scale type, the author arrives at the restrictions imposed on the mathematical form of the laws which can connect the variables. In particular, if the independent and the dependent variables are both measured on ratio scales (where the zero point is fixed), the formula connecting them can be only a power formula: $u(x) = \gamma x^{\beta}$. If the independent variable is measured on a ratio scale, and the de-

pendent variable on an interval scale, the formula must be logarithmic: $u(x) = \gamma \log x + \beta$, etc. Moreover there can be *no* formula connecting an independent variable measured on an interval scale and the dependent one measured on a ratio scale. (Note: physical laws in which the independent variable is "temperature" must be so expressed that this variable is a temperature *difference* or else *absolute* temperature, both measured on a ratio, not an interval scale.)

The author discusses also physical laws expressed by formulas which do not fit into the forms he prescribes for the cases where the variables are measured on specific scale types. He shows that in all such cases the actual variable is dimensionless, i.e., a pure number. Such a variable admits only the identify transformation and therefore the formulas connecting such variables are subject to no restrictions. Indeed in all physical formulas one finds constants which serve as coefficients of variables and have units reciprocal to those of the variables so as to give dimensionless variables in the products. In such instances the laws may be of any form (note that probabilities, information measures, etc. are all dimensionless). The second exception obtains if the variables are discrete or if the functional relations are discontinuous, in which case the derived restrictions do not apply. (*AR*)

SANTOSO, R. SLAMET IMAN. **The social conditions of psychotherapy in Indonesia.** *Amer. J. Psychiat.*, 1959, 115, 798–800.

The author, a Javanese Western-trained psychiatrist, discusses the problem of applying Western-oriented psychotherapeutic techniques to different ethnic groups in Indonesia. Difficulties arise in using languages to which psychotherapeutic concepts are foreign and because of the "materialistic-organic" orientation of Indonesian medicine.

Santoso's description of loss of paternal authority as a cause of instability and neurosis has its counterpart in the attitudes of many about changing roles and identity in our own culture. Conflict about attitudes toward religion, the pressures of education, and the rapid upward mobility for the new intelligentsia are other unsettling factors in the culture.

The author contrasts the problem of treatment of Javanese (i.e., necessity of understanding the language thoroughly since certain syncretic elements are a reflection of the Javanese thought pattern) and that of treating the Minangkabauw (i.e., the necessity of understanding a matriarchal domestic system and its effects on the psychic development of the people). He concludes with an emphasis on an anthropologic orientation to psychotherapy. (*JD*)

The impression is to the writer what experimentation is to the scientist, with this difference, however, that for the scientist the labor of the intellect precedes and for the artist it follows.

The Maxims of Marcel Proust

ABOUT THE AUTHORS

Harold Garfinkel ("The Rational Properties of Scientific and Common Sense Activities") is affiliated as associate research sociologist with the department of psychiatry, in the School of Medicine at UCLA. The recipient of a senior research fellowship from the National Institutes of Health, he is spending the current year as a research fellow in the department of social relations at Harvard. He was awarded the fellowship for a five year program of studies on how normal, taken-for-granted interpersonal relations are established, maintained, altered, and restored in the activities of every day life. This program has involved research on how persons assign status to others; the socially controlled production of patient careers in a psychiatric outpatient clinic; decision making in common sense situations of choice; and common sense activities, common sense knowledge, and the method of common sense thinking both as specific topics and as features of sociological inquiry. He states that he has been particularly influenced in his work by the writings of Talcott Parsons and of the late Alfred Schutz. The author of several articles, he received his M.A. degree from the University of North Carolina and his Ph.D. in Sociology from Harvard.

Bert F. Green, Jr. ("IPL-V: The Newell-Shaw-Simon Programming Language") received his B.A. degree in mathematics from Yale and his M.A. and Ph.D. degrees in Psychology from Princeton. At present, he is leader of the psychology group at the Lincoln Laboratory of M.I.T., and is interested in research on the applications of digital computers to psychology, including pattern recognition and man-machine communication. He writes that "At Princeton, I had an Educational Testing Service psychometric fellowship and was primarily interested in psychometrics. Since getting my Ph.D., I have been at M.I.T., working on various aspects of man-machine cooperation. It was here I met and fell in love with digital computers, and have been advertising their benefits to psychologists ever since! In the summer of 1958, I participated in an SSRC-sponsored conference at the RAND Corporation on the simulation of human behavior, and was converted to the Newell-Shaw-Simon programming system, as well as to their methods of studying human problem solving." Among other publications, he is a co-author of a forthcoming article in the *American Journal of Psychology* entitled "The detection of statistically defined patterns in a matrix of dots."

John W. Gyr ("An Investigation Into, and Speculations About, the Formal Nature of a Problem-Solving Process") is associate research psychologist at the Mental Health Research Institute of The University of Michigan, where he received his Ph.D. degree in social psychology. He states that his present research interests concern "the use of computers for the simulation of human problem solving. Psychologists now have the opportunity to take advantage of the fact that computers may be used to simulate (in a formal sense) the human problem-solving process, once this process has been formulated in an appropriate mathematical language. The study of complex behavior, involving the interaction over time of many parameters, and the exhaustive tests of behavior models designed to understand such behavior, now becomes a possibility." He is co-author of a book entitled *Teaching Comprehensive Medical Care*, Harvard University Press, 1959, and the author of articles which have appeared in *Human Relations* and *Psychological Reports* in the areas of decision making, conflict, and philosophy of education.

Paul W. Hagensick ("Logic by Machine: Programming the LGP-30 to Solve Problems in Symbolic Logic") is an assistant professor of philosophy at Ohio University. He writes that "Two years ago, I was contacted by Professor Lawrence Gallagher, a physicist, who had been considering how the LGP-30 might be programmed to solve problems in symbolic logic. We discussed this question at considerable length and, having resolved the theoretical problems, began working out the actual programs. This paper is the product of this activity and represents an intermediate stage in our researches. With funds granted by the Ohio University Research Committee, we plan to investigate the limits to which the computer program can be extended when considering other areas of symbolic logic." In addition to this work, Professor Hagensick is cataloguing a library of nearly 1300 books in the history of science from 1500 to the present that have been presented to Ohio University; is undertaking research on various topics in the works of Galileo, particularly those concerned with mechanics and the development of scientific laws; and is preparing a text book in elementary logic for use at about the junior

level. He received his B.A., M.A. and Ph.D. degrees from the University of Wisconsin.

Irving Lorge ("Group and Individual Performance in Problem Solving Related to Previous Exposure to Problem, Level of Aspiration, and Group Size") comments: "In our studies of group and individual performance in actual field situations, it was not unusual to see a person sit on a beam and say, 'Now this is the way to do it and if you don't do it my way, we'll not do it at all.' This is perhaps the democratic effect on problem-solving which we have discovered in other ways. We are not always certain that the reason a problem is not solved satisfactorily by a group is not due to the fact that some of them are impeding the remainder of the group from showing its best sense." The author of more than 200 publications in various fields, Dr. Lorge is the executive officer of the Institute of Psychological Research at Teachers College, Columbia University.

Joseph J. Schwab ("What Do Scientists Do?"), the William Rainey Harper Professor of Natural Sciences at the University of Chicago, has published a number of papers on genetics, the nature of inquiry, and education. His present research interests center on patterns of inquiry and behavior of inquirers in the biological and behavioral sciences. He writes "The paper, What Do Scientists Do?, is part of a long time study of inquiry. The format and emphases of this paper arose from observation of the methodological conflicts and controversies among the Fellows at the Center for Advanced Study in the Behavioral Sciences, 1958–59, with special interest in the dawning concern of academic psychologists for the quality of inquiry, here called 'validity.' "

Herbert Solomon ("Group and Individual Performance in Problem Solving Related to Previous Exposure to Problem, Level of Aspiration, and Group Size") received his M.A. degree from Columbia and his Ph.D. degree from Stanford University in 1950. Currently professor and executive head of the department of statistics at Stanford, he is interested in research on mathematical statistics and mathematical methods in the behavioral sciences. He is the author of various articles in such journals as *Annals of Mathematical Statistics*, *Psychometrika*, and *Management Science*.

Hans H. Strupp ("Some Comments on the Future of Research in Psychotherapy"), a previous contributor to *Behavioral Science* (January 1958), is director of psychological services and associate professor of psychology in the department of psychiatry at the University of North Carolina. He received his Ph.D. degree from George Washington University in 1954 and a certificate in applied psychiatry for psychologists in 1952 from the Washington School of Psychiatry. He is engaged in research in psychotherapy, particularly concerned with the question: what is therapeutic in psychotherapy and how can we be sure that the ingredients to which we attribute therapeutic change are the real ones? He comments: "This paper reflects a very genuine puzzlement and concern about the future of research in psychotherapy. I would like to believe that systematic research will extend the frontiers of knowledge in psychotherapy, but there are times when I entertain serious reservations and doubts. I am inclined to believe that our research methodology lags far behind our clinical insights. Researchers in psychotherapy are still perceived as enemies by many psychotherapists and the suspicion that the investigator (note the dual meaning of the term!) ultimately wants to hold up psychotherapy to ridicule is never far. I think this is a very unfortunate (if sometimes not totally unjustified) reaction. I, for one, am convinced that the insights which have come and will come from the psychoanalytic method are our best hope. Our task is to give them a more solid empirical foundation. Can researchers and therapists become allies and perhaps even friends? Everyone might benefit!"

Incidental Intelligence:

Near Petionville, Haiti, is a supermarket named "The Minimax Supermarket."

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A crystallized tear
From the eye of a deer—
Bitten by a snake—
Made people well.
So who can foretell
What cures placebos make?

A CONTRIBUTION TO A HISTORY OF THE PLACEBO EFFECT

by Arthur K. Shapiro[1]

New York University College of Medicine

"Those who forget the past are destined always to repeat it."

Santayana

INTRODUCTION

INTEREST in the placebo effect has increased considerably in recent years, more papers on the placebo having appeared in the last four years (35 during 1954–1957) than in all previous years (21 between 1900 and 1954). (See Figure 1.) Indices, journals, texts, and abstract journals are increasingly including the word placebo in their list of references; the word placebo appears more frequently in the title of articles (Table 1); and it is becoming a commonplace in research, as it has been a commonplace in medicine for a much longer period of time. A placebo control is included in double blind studies which also are appearing more frequently in scientific literature.

The sharp increase in "psychiatric"[2] interest in the placebo can be seen illustrated graphically in Figure 1, more articles on this subject having appeared in the last two years (9 during 1956–1957) than in all previous years combined (5 between 1900 and 1956). These last two years have been the only years in which the number of "psychiatric" articles (9) has equaled the number of "non-psychiatric" articles (9). Many of these articles comment on the importance of studying the placebo effect. This increased interest is indicative of an important new trend in psychiatry, medicine, and the other sciences.

This paper will be concerned primarily with historical considerations. Special emphasis will be placed on the placebo and placebo effect in this history, and on the history of the use of the placebo and the development of the placebo effect concept. Some of the major contributions to an understanding of this concept will be noted and outlined. Some of the conclusions, implications, and questions stimulated by these historical considerations will be discussed.

The paper will not be concerned primarily with the use and ethics of placebos, with the evidence for the existence of the placebo effect, the magnitude of the placebo effect, or an analysis of the placebo effect. It will not be concerned primarily with the theories that have been advanced to explain the placebo effect, or theories which are suggested by the evidence. This will be reported elsewhere (Shapiro, a, b, c).

MEDICAL (NON-PSYCHIATRIC) HISTORY AND THE PLACEBO EFFECT

Let us begin with the "non-psychiatric" medical history. In the following section the "psychiatric" history will be presented.

Sir William Osler (1905) felt that, ". . . the desire to take a medicine is one feature which distinguishes man, the animal, from his fellow creatures." It is probable that

[1] This paper was completed at the Massachusetts Mental Health Center, Boston Psychopathic Hospital, and Harvard Medical School.

[2] The term "psychiatric" refers to psychiatric and psychological articles or interest. The term "non-psychiatric" refers to all other medical specialties and workers in areas such as pharmacology, physiology, etc.

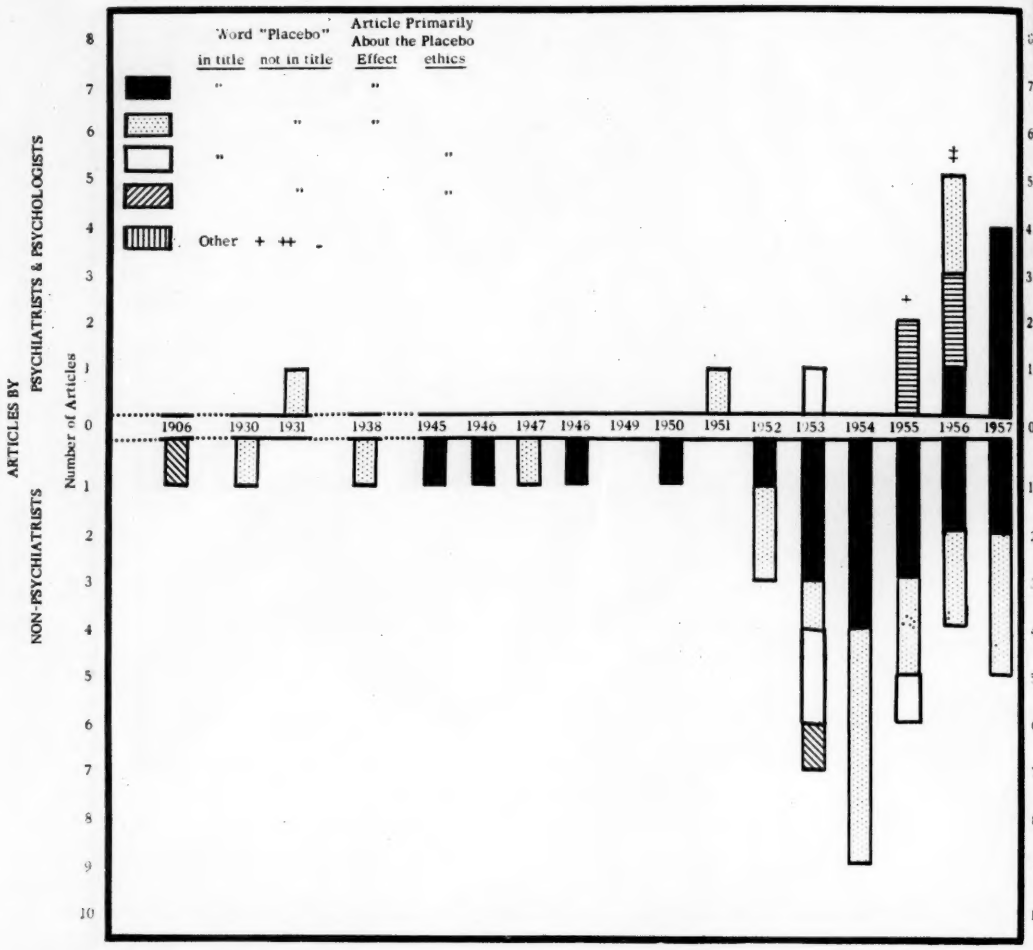

FIG. 1. Papers on the placebo effect and ethics according to year and whether the author was a psychiatrist, psychologist, or non-psychiatrist and whether the word "placebo" appears in the title.*

* Criteria for inclusion of papers in this list and references for the various years will be supplied by the author upon request.

† Both articles with placebo in the title and written by psychiatrists but one appearing in a medical journal (Hofling, 1955) and the other in a psychological journal (Abramson et al., 1955).

†† Both articles with placebo in the title, one written by a psychiatrist and appearing in a medical journal (Fisher & Olin, 1956), the other written by a psychologist as principal author and a psychiatrist as second author and appearing in a psychological journal (Rosenthal & Frank, 1956).

whatever beneficial effects accrued to man's first medication could only have been due to the placebo effect (Modell, 1925; Rivers, 1927). This may be defined at the present time as the psychological, physiological, or psychophysiological effect of any medication or procedure given with therapeutic intent, which is independent of or minimally related to the pharmacological effect of the medication or to the specific effects of the procedure, and which operates through a psychological mechanism. Although the pharmacologic effect of a drug may have been deleterious or of little consequence to the organism, its effect could have been beneficial nevertheless. Indeed, this is the history of medical treatment for the most part until relatively recently, since almost

TABLE I

The Number of Papers Indexed in Various References With the Word "Placebo" in the Title, According to Year and Whether the Author Was a "Non-Psychiatric" M.D. (M.D.) or a "Psychiatric" M.D. or Psychologist (PSY.)*

Year	Current List of Medical Literature		Quarterly Cumulative Index Medicus		Excerpta Medica-Neurology Psychiatry		Digest of Neurology and Psychiatry		Psycho-logical Abstracts	
	M. D.	PSY.	M. D.	PSY.	M. D.	PSY.	M. D.	PSY.	M. D.	PSY.
1957							-	1		
1956	3	3			-	-	-	1	-	-
1955	6	-	-	1	-	-	-	-	-	-
1954	2	-	5	-	-	-	1	-	1	-
1953	4	-	3	-	-	-	-	-	-	-
1952	1	-	-	-	-	-	-	-	-	-
1951	-	-	-	-	-	-	-	-	-	-
1950	3	-	1	-	-	-	1	-	-	-
1949	-	-	-	-	-	-	-	-	-	-
1948	-	-	1	-	-	-	-	-	1	-
1947	1	-	-	-			-	-	-	-
1946	-	-	1	-			-	-	-	-
1945	-	-	1	-			-	-	-	-
1944	-	-	-	-			-	-	-	-
1943	-	-	-	-			-	-	-	-
1942	-	-	-	-					-	-
1941	-	-	-	-					-	-
1940	-	-	-	-					-	-
TOTALS	20	3	12	1	0	0	2	2	2	0

*REFERENCES NOT IN EXISTENCE, AVAILABLE OR REVIEWED ARE INDICATED BY A BLANK SPACE. FOREIGN ARTICLES HAVE NOT BEEN INCLUDED.

all medications until recently were placebos (Houston, 1938; Pepper, 1945).

Patients in ancient Egypt, according to the Ebers Papyrus, in 1500 B.C., were often treated with medication such as "lizard's blood, crocodile dung, the teeth of swine, the hoof of an ass, putrid meat and fly specs" (Findley, 1953). No treatments of specific value are found in all the pages of Hippocrates (Houston, 1938). Despite this, and the continued prescription of the "flesh of vipers, the spermatic fluid of frogs, horns of deer, animal excretions, holy oil" (Leslie, 1954) and other bizarre substances, the physician continued to be a useful, respected and a highly honored member of society.

The history of medical treatment concordant with scientific progress in general, is at the same time incredible (Castiglioni, 1946; Garrison, 1921; Haggard, 1929; Haggard, 1933; Haggard, 1934; Goldwyn, 1958; Major, 1954; Major, 1955; Rapport and Wright, 1952; Rivers, 1927; Sigerist, 1958; Zilboorg, 1941). Four of the most famous medications that were used by physicians up to the 16th and at times during the 18th centuries were: the fabled unicorn's horn to detect and protect against poisons in wines, bezoar stones as antidotes for poisons of all types, theriac as a universal antidote, and powdered Egyptian mummy to heal wounds and as an almost universal remedy. These medications were very expensive and only the wealthy could afford them. Unicorn's horn usually came from the ivory of the narwhal or elephant. Bezoar stones, according to legend, were a crystallized tear from the eye of a deer bitten by a snake. In reality, they were a concretion or gallstone found in the stomachs and intestines of animals such as the goat. Theriac dates back to the experiments of Mithradates and was used up to one hundred years ago. Although the main ingredient was the flesh of vipers, it contained 37 to 63 ingredients, all of which were worthless, and which often required several months to concoct. Several official conferences complained about the adulteration of the

powdered mummy from Egypt and the impurity of the unicorn's horns. Powdered mummy presumably came from ground-up mummy in Egypt. Mattioli (an antidote containing 230 ingredients), usnea (the moss scraped from the skull of a hung criminal), crocodile dung, fly specs, eunuch fat were only a few of the more unpalatable drugs. Medical reasoning was primitive: the lungs of the fox were given to consumptives because the fox was a long-winded animal; the fat of a bear, a hirsute animal, was given to those suffering from baldness. Despite the ignorance and superstition, the physician must have benefited his patient because he continued to be held in high esteem.

Consider the treatment that Charles II endured by the physicians of his day: "A pint of blood was extracted from his right arm, and a half-pint from his left shoulder, followed by an emetic, two physics, and an enema comprising fifteen substances; the royal head was then shaved and a blister raised; then a sneezing powder, more emetics, and bleeding, soothing potions, a plaster of pitch and pigeon dung on his feet, potions containing ten different substances, chiefly herbs, finally forty drops of extract of human skull, and the application of bezoar stone; after which his majesty died." (Van Dyke, 1947; for a more complete description, see Haggard, 1929).

Astute observers of their time, such as Montaigne (1946) in the sixteenth century, observed that doctors, in general, were a danger to their patients. Earlier, in the twelfth century, Maimonides (1957), implied this in his statement, "I call him a perfect physician who judges it better to abstain from treatment rather than prescribe one which might perturb the course of the malady." Moliere's satires in the seventeenth century on the medicine of his time are well-known (Garrison, 1921). As late as the seventeenth century, a contemporary of Moliere, Robert Boyle, the father of modern chemistry, after expunging many questionable remedies from the revised pharmacopoeia, included the sole of an old shoe "worn by some man that walked much" which was to be ground into a powder and taken for stomach ache (Haggard, 1929). Oliver Wendell Holmes (1891) said as recently as 1860, that nearly all the drugs then in use should be thrown "... into the sea where it would be the better for mankind, and all the worse for the fishes." Despite this, sick patients continued to submit to purging, puking, cutting, cupping, blistering, bleeding, freezing, heating, sweating, and shocking.

In 1794, Dr. Ranieri Gerbi, a professor at Pisa, published a manuscript describing a miraculous cure for toothache due to any cause which lasted for a whole year. A worm species, called curculio antiodontaligious, was crushed between the thumb and forefingers of the right hand. The fingers then touched the affected part. An investigatory commission found that 431 of 629 toothaches were stopped immediately. Later, Dr. Carradori, court physician at Weimar, advanced the discovery by substituting a more pleasant ladybird, and an official commission confirmed the immediate relief of toothache in 65–70 per cent of cases. Soon thereafter, an English paper published the following prescription for toothache: "fill your mouth with milk and shake it until it becomes butter, in this way at least three out of four toothaches cease immediately and without fail." (Volgyesi, 1954). This prescription, although pharmacologically ineffective, was more palatable than Pliny's prescription that a mouse be eaten once a month to prevent toothache (Haggard, 1934), or the use of urine as an analgesic mouthwash.

Mistletoe was thought by the ancient druids to have curative virtues against St. Anthony's fire (erysepelas), corns, frostbite, muscular atrophy, apoplexy, infertility, chorea, and bubonic plague (*Spectrum*, 1957, b). It was Galen's drug of choice and thought to be a plant that grows on the oak and hence cannot fall, and naturally should be a specific for the falling sickness (Lennox, 1957). In 1719, however, Colbatch (1719) found it as effective when grown on the lime tree. By the 17th century its powers had spread and it was thought to be effective against edema, impotence, leprosy, syphilis and other diseases. It was thought to be

effective against epilepsy from the 17th century and was used by Sir Charles Locock, physician to the queen, who thought epilepsy might be caused by crowded teeth, the practice of onanism, and by a hysterical form connected with the menstrual period (Lennox, 1957)—all of which are completely unrelated to the disease.

A Dr. Raymond at the Salpetriere in Paris found 70 per cent of patients suffering from a variety of diseases, including tabes, were treated successfully by suspending them by their feet, causing the blood to flow into their heads. A collaborator of Bernheim at Nancy, a Dr. Haushalter, reversed the procedure and suspended the patient's head upwards and obtained a similar per cent success in a variety of organic and nervous diseases (Volgyesi, 1954).

One could expand at considerable length on other such examples. Today we know that the effectiveness of these procedures and medications was due to the placebo effect, although little is known about how the placebo effect is effective. Let us continue to explore the historical past in order to learn how not to repeat it.

The placebo effect and the doctor-patient relationship

Although medicine was integrally related to the finest scientific, religious, cultural, and ethical traditions in most periods of history, and despite the ephemeral and/or quite quantitative appearance of drugs or procedures which were truly helpful, one may ask how physicians maintained their position of honor and respect throughout history in the face of thousands of years of prescribing what we now know to be useless and often dangerous medications? Indeed, this would have been a major accomplishment of the physician were it not for the fact that despite the uselessness of the drugs and procedures, nevertheless, the physician truly helped the patient. It was not to the specific medications, however, that the patient responded, but to something inherent in the doctor-patient relationship.[3] Findley (1953) asks, "... if it can fairly be said that scientific medicine began only seven or eight decades ago, what of those who practiced before? Were they ignorant, unscrupulous, fraudulent?" He goes on to say, "... that the physician is a vastly more important institution than the drug store," and that even today, "Despite the scientific achievements of this century the physician himself is still the most important therapeutic agent." According to Findley, the effectiveness of this placebo effect lies in the action, ritual, faith and enthusiasm on the part of both the doctor and patient.

Houston (1938), in an early article, discusses the doctor as a therapeutic agent, describing early medicine as a symbol related to the placebo effect. He states that in the early history of medicine the relationship between doctor and patient comprised all that the doctor had to offer the patient. The medicines used were placebos, and, "The great lesson, then, of medical history is that the placebo has always been the norm of medical practice, that it was only occasionally and at great intervals that anything really serviceable, such as the cure of scurvy by fresh fruits, was introduced into medical practice.... Their skill was a skill in dealing with the emotions of men. They themselves were the therapeutic agents by which cures were effected. Their therapeutic procedures, whether they were inert or whether they were dangerous, were placebos, symbols by which their patients' faith and their own was sustained." He continues, "The history of medicine is a history of the dynamic power of the relationship between doctor and patient. Through centuries when doctors were doing more harm than good this dynamic force has sustained the medical pro-

[3] Another factor contributing to the "success" of these physicians in the past must be considered. Spontaneous recoveries and remissions "can" occur in the course of almost any illness and "may" be unrelated to the administered drugs, doctor-patient relationships and placebo effects. It is very difficult, however, to isolate this factor because of the potential placebo effect in every therapeutic relationship. In addition, the belief of the physician and/or the patient that recovery is a consequence of some activity of the physician would tend to increase the effect of placebos in general. Although this factor must be postulated, it does not negate what is thought to be the more important influence of the doctor-patient relationship in the history of medical treatment.

fession in the esteem of their clientele, it has inspired their fellow citizens with such faith in its values that they were willing to give economic support to the doctor." Houston relates the efficacy of the placebo to the faith of the doctor and patient in the medication, to the faith that the doctor is able to impart to the patient, and to the ameliorative effect of action which the doctor provides in some form.

This view is reiterated by Modell (1955): "It would appear that this placebo action is the one constant in the long history of medical practice. It is the one common denominator, for instance, between the treatment of the Egyptian physician who, in the Papyrus Ebers, prescribed a draught of one-thirty-second part of Tail-of-Mouse with Honey, one third, for cooling the anus, and the treatment of the modern physician who prescribes penicillin for pneumonia. Placebo effect was present in the administrations of the first tribal witch doctor and is equally active today, since it is a component . . . not only of medication, but of every phase and kind of medical treatment."

The frequent reference to the importance of the "art of medicine" implies an understanding of the placebo effect (Bromberg, 1954; Bromberg, 1958; Hailman, 1953; Modell, 1955; Parsons, 1951; Pepper, 1945). Though none of the drugs listed by Hippocrates have proven to be of any usefulness, his observation on the "art of medicine" implies an understanding of the placebo effect and his psychotherapeutic acumen was extremely pertinent (Block, 1957). The famous admonition, loosely paraphrased, "You should treat as many patients as possible with the new drugs while they still have the power to heal," implies a knowledgable appreciation of the placebo effect, a statement which has become so famous that it has been attributed to Trousseau (Findley, 1953; Koteen, 1957; *Spectrum*, 1957, a), Osler (Wayne, 1956), Sydenham (Elkes, 1954) and Nolan D. C. Lewis (Sabshin, 1956). Another historical fact which implies an effect of the placebo has been noted by many authors (Bernheim, 1889; Bromberg, 1954; Grinker, 1958; Horwitt, 1956; Janet, 1925; Sabshin and Ramet, 1956). Findley (1953) says, "One drug after another has its little day of popularity, then sinks into oblivion. Sheaves of paper by well-intentioned authors testify to their usefulness, but they are quickly replaced by others. This familiar phenomena is perhaps the strongest argument for the validity of placebo therapy, for it means faith endows drugs or treatments with powers which they themselves do not possess." The importance of faith is reflected in the fact that one of the best educated, major religious groups in the United States is able to deny the rational efficacy of any treatment or medicine and to assign all treatment benefits to faith. Houston (1938) remarks on the many reminders of the historical past in the contemporary successful existence of the nostrum vendors, chiropractors and myriads of other varieties of quackery. He also notes that no one considers applying the principles of osteopathy to animals, where it would certainly fail, because the principles are effective only when they involve a human interpersonal relationship, [although it is possible that animals are capable of "placebo-like" reactions also (Gliedman, Gantt, & Teitelbaum, 1957; Wolff, 1946)].

When one considers that the normative history of medical treatment, until relatively recently, has been the history of the placebo effect, one is amazed to find a veritable curtain of silence about it. Houston (1938) likens the pages of medical history to the log of an old-fashioned ocean voyage, in which it was noted that on such a day a whale spouted, or a flying fish was sighted, or a bit of driftwood seen, but in which no mention is made of the huge prevailing fact, that what was constantly seen, almost exclusively, was the unending green waste of water. This circumambient ocean is by analogy the placebo effect.

The Countess of Chinchon is erroneously credited with introducing cinchoma bark, which contains quinine, as a treatment for febrile infections in 1638. Sydenham, by demonstrating that it was specific only for fever of malarial origin, contributed to the end of Galenism and the beginning of scientific medicine (Garrison, 1925; Haggard,

1934; Major, 1954; Major, 1955; Pepper, 1945). It may be considered that this was the first drug that was not a placebo (Findley, 1953; Houston, 1938; Pepper, 1945) because previous to this there was no way to distinguish a placebo from a non-placebo (Pepper, 1945). But even after this period, and even after the beginning of scientific medicine which began seven or eight decades ago, (Findley, 1953; Rivers, 1927; Van Dyke, 1947), one cannot escape noticing the continued resistance to the use of the word "placebo", or the concept underlying the word, in medical parlance, dictionaries, and texts. This is illustrated in the paper by Pepper (1945), which is of interest in that it was the first paper devoted exclusively to the placebo, with the word "placebo" in the title.

History of the word "placebo"

Pepper (1945) eloquently traces the history of the word "placebo" (see also Webster's, 1940, and the Oxford, 1933, Dictionaries). It is the first person singular of the future indicative of the Latin verb "to please"; the word "placebo" itself being equivalent to the phrase "I shall please." The first use of the word dates back to at least the thirteenth century. It appears in the Vespers for the Dead in the Roman Catholic service, the 114th Psalm, in the Vulgate, beginning, "Placebo Domino in regione vivorum. . . ." It was used in the sense of "I will walk before . . . " or "I will please. . . ." It is then found in phrases suggesting sychophancy and servility, and as a noun meaning a flatterer, sychophant or parasite. It was used by Chaucer in both senses, and a character of this type is given Placebo as a proper name. Sir Walter Scott used it in the sense of a soothing sentiment. It is defined as "a commonplace method of medicine" in the 1787 edition of Quincy's *Lexicon* and in the *Philadelphia Medical Dictionary* published in 1808 by John Redman Coxe. Pepper points out that this definition may illustrate the earliest stage of doubt concerning the efficacy of prescriptions of those days, and an approach to the frank admission of a quarter of a century later which appeared in the 1811 edition of Hooper's *Medical Dictionary* with the definition of the placebo as "an epithet given to any medicine adopted more to please than to benefit the patient."

Thus the placebo appears in medical terminology five hundred years after it was first used in other ways. Let us not be too surprised at this belated appearance, because despite the placebo being freely prescribed during the nineteenth and twentieth centuries, the word placebo did not appear in the index of H. C. Wood's *Therapeutics* whose fourteen editions covered the period from 1875 to 1908, and because, as we shall see later, the resistance to and silence about this important therapeutic agent continues into the more recent history of the placebo.

Placebo effect before 1900

Difficulties are encountered when reviewing the literature prior to 1900. This literature is massive and the indexing is inadequate. The terms placebo and placebo effect were hardly used prior to 1900. This review, therefore, cannot be extensive or exhaustive.

There are, however, many indications that physicians and others of the past (even from earliest times) were cognizant of "placebo-like" phenomena, although this was not referred to as the placebo effect, exhaustively studied or extensively written about. Certainly the full implications of this phenomena were not appreciated.

The observations of Maimonides, Montaigne, Moliere, and Trousseau have been mentioned previously.

Pierre Pomponazzi, an Italian philosopher, uttered these words in the 16th century: "We can easily conceive the marvelous effects which confidence and imagination can produce. . . . The cures attributed to the influence of certain relics, are the effect of this imagination and confidence. Quacks and philosophers know that if the bones of any skeleton were put in place of the saint's bones, the sick would none the less experience beneficial effects if they believed that they were near veritable relics." (Tuke, 1886).

Paracelsus commented in the fifteenth century: "Whether the object of your faith be real or false, you will nevertheless obtain

the same effects. Thus, if I believe in St. Peter's statue as I would have believed in St. Peter himself, I will obtain the same effects that I would have obtained from St. Peter;—but that is superstition. Faith, however, produces miracles and whether it be a true or a false faith, it will always produce the same wonders." (Charpingnon, 1864).

It should be noted, however, that these observations are directed not to medical practice, but to religious faith and mysticism as treatment. To Paracelsus' credit belongs his vitriolic opposition to the polypharmacy of his day, the observation that medicine killed and that nature heals. He, however, espoused his own brand of occultism and clung to his own placebo medication, e.g., sympathetic powder, which cured the injuries of a wounded person when applied to the blood-stained garments (Garrison, 1921). Paré was notable for his contributing to the abandonment of many venerable beliefs, e.g., Egyptian mummy, unicorn horn, bezoar stones and boiling oil for cauterization of wounds. He substituted his own placebo for the latter eventually with a concoction consisting of earthworms steeped in turpentine and oil of puppies and lillies (Garrison, 1921).

Oliver Wendell Holmes (1891) first presented his very interesting lecture on *Homeopathy and its Kindred Delusions in 1842*. Although his main concern is with a criticism of homeopathy, he discusses the Royal cure of the King's Evil, the Weapon Ointment, Sympathetic Powder, the Tar-water panacea of Bishop Berkeley and Perkins' Metallic Tractors. He asseverates how patients can benefit "through the influence exerted upon their imaginations," and how they, "all display in superfluous abundance the boundless credulity and excitability of mankind upon subjects connected with medicine." He examines these phenomena in order to illustrate the ease with which numerous *facts* are accumulated to prove the most fanciful and senseless extravagances, and the inefficiency and incompetency of persons without medical knowledge despite wisdom, honesty, and accomplishment. He is cognizant of the role of spontaneous remission in the natural course of disease, citing a figure of 90 per cent recovery in patients seen in general practice, "provided nothing were done to interfere with the efforts of nature." He is familiar with elemental probability theory and mentions several primitive attempts at controlled studies.

Although Holmes does relegate the majority of drugs then in use to the bottom of the sea, for the most part, he addresses himself to the abuses of the past and those at the periphery of medicine, and not to the mainstream of medical practice of his day. However, there are some implications that he does address himself to the medical practice of his day indirectly and by analogy. This is implied to some extent in the article and especially in the preface to the article.

Although many authors in the past (as today) have given some indication that they were cognizant of "placebo-like" effects (the degree of sensitivity varying considerably), it is striking how this was no guarantee against them becoming subject to a belief in a placebo of their own.

This is dramatically illustrated by the cogent observation of Quimby in 1859, who said, "But through a great many mistakes, and the prescription of a great many useless drugs, I was led to re-examine the question, and came in the end to the position I now hold: the cure does not depend upon any drug, but simply upon the patient's belief in the doctor or the medicine." (Janet, 1925). P. P. Quimby was the immediate predecessor of Mary Baker Eddy and Christian Science.

Paré believed in the puppy ointment and Paracelsus espoused occultism. Trousseau, who was able to say "You should treat as many patients as possible with the new drugs while they have the power to heal," was able to claim diseases (such as neuralgia, nervous dyspnoea, angina pectoris and rheumatism) to be cured, modified and rapidly checked by the use of magnets "which it is impossible to refer to the patient's imagination" (Trousseau, 1833).

William Osler was a therapeutic nihilist who battled the irrational polypharmacy of his day. When discussing "Psychical methods of cure, in which faith in something is

suggested to the patient," (Osler, 1892), he approvingly quotes Galen as saying, "he cures most in whom most are confident," and Paracelsus encouraging his patients "to have a good faith, a strong imagination, and they shall find the effects." He continues, "Faith in the gods or in the saints cures one, faith in little pills another, hypnotic suggestion a third, faith in a plain common doctor a fourth. . . . Faith in us, faith in our drugs and methods, is the great stock in trade of the profession . . . the touchstone of success in medicine . . . and must be considered in discussing the foundation of therapeutics . . . a most precious commodity, without which we should be very badly off." He points out that doctors do not enjoy a "monopoly in the faith business," and that, "While we doctors often overlook or are ignorant of our own faith-cures, we are just a wee bit too sensitive about those performed outside our ranks." Despite the latter, Osler used cupping, leeching and venesection (and other doubtful medicinal substances) in the treatment of pneumonia, stating, "a timely venesection may save life . . . relieving the pain and dyspnoea, reducing the temperature, and allaying the cerebral symptoms" (Osler, 1892).

It is appropriate to remember the wise words of Janet (1925): "It is only too easy to make fun of stories of miraculous cure. Not merely are sceptics prone to use this weapon of ridicule, but the faithful do the same thing. The devotees of a religion are strongly inclined to attack kindred superstitions. Nothing will persuade the adepts of Lourdes that the miracles worked at the shrine of Aesculapius were genuine. Who could induce the admirers of animal magnetism to take a serious view of such miracles as those of Lourdes: Each one attacks his neighbour, and is quite unaware that his criticisms rebound upon himself."

Placebo effect from 1900 to 1945

Very few articles were written on the placebo between 1900 and 1945. It is difficult to trace such articles because the topic has never been indexed in the Index of the Surgeon General's Library, and not in the Quarterly Cumulative Index Medicus prior to 1945, or in the Current List of Medical Literature prior to 1946. (Table 1) One would have to scour mountains of literature to completely cover the literature during this period. This literature was covered, insofar as it could be, by tracing each reference to its source that came to the author's attention, and by reviewing subjects such as the doctor-patient relationship, therapeutics, faith cures, and suggestibility. Several articles were found which discussed the placebo and placebo effect and which characteristically did not have the word placebo in the title.

Cabot (1906) discusses the ethics of the "Nostrum Evil" referring to the placebo as quackery. In the same year, Fantus (1906), in his text on pharmacy and prescription writing, briefly mentions the positive indications for the use of placebos.

Rivers (1908) studied *The Influence of Alcohol and other drugs on Fatigue* in which he used inert material, not referred to as a placebo, as a control. The latter was designed to taste and appear indistinguishable from the experimental drug. The subjects, or when the experimenter was a subject, had no knowledge of the substances they were receiving. It is one of the earliest anticipations of the double blind procedure. Many methodological variables are considered. It represents a very early understanding of (and greater sophistication than many contemporary investigators about) some of the powerful effects of the placebo in experimentation. It is interesting that no statistical analysis was made of the extensive data, because as recently as 1908 this methodological tool was unavailable. Rivers' (1927) erudition and prescience again appears in his posthumous *Medicine, Magic and Religion*, which was written for the most part prior to 1920. He says, "The action of suggestion can never be excluded in any form of medical treatment, whether it be explicitly designed to act upon the mind or whether ostensibly it is purely physical in character. . . . If we confine our attention to our own culture, it is only within the last fifty or sixty years that there has been any clear recognition of the vast importance of the mental factor in the production and

treatment of disease, and even now this knowledge is far from being fully recognized either by the medical profession or the laity. . . . Though remedies acting through the mind were probably the earliest to be employed by Man, the knowledge that the remedies act in this way is one of the most recent acquirements of medicine. . . . Few can now be found who will deny that the success which attended the complex prescriptions, and most of the dietetic remedies of the last generation, was due mainly, if not entirely, to the play of faith and suggestion. The salient feature of the medicine of today is that these psychical factors are no longer allowed to play their part unwittingly, but are themselves becoming the subject of study, so that the present age is seeing the growth of a rational system of psychotherapeutics."

Ayman (1930) discusses the concept of the placebo without mentioning the word in a splendid paper in which he evaluates the therapeutic results in 35 papers on essential hypertension. He found that in every paper complete or partial symptomatic relief was described. Up to 85 per cent reduction in blood pressure was reported. Mistletoe, diathermy, watermelon extract, and Nauheim baths were some of the therapeutic agents listed. Ayman treated forty patients with drops of dilute HCl t.i.d. and 82 per cent showed definite improvement. Many minor and some major side effects were reported. Ayman believed that the common element was the enthusiastic giving or doing of something to the patient.

Much of the material in the very fine article by Houston (1938) has been mentioned previously. In addition, the word "placebo" is used extensively, and some of the factors involved in the placebo effect are discussed. Fantus (1938) expands in greater detail on his previous statement in 1906. He again advises candor and psychotherapy where possible, but briefly discusses the situations in which placebos are indicated—patients with low intelligence and imaginary illnesses. He decries the use of expensive placebos and the widespread use of medication such as vitamins that are prescribed in the spirit of the placebo.

No other articles have come to the author's attention prior to 1945 except for those by psychiatrists which will be discussed later.

As can be seen from Figure 1, prior to 1952, there was less than one article on the placebo effect per year. Despite the demonstration by Ayman as early as 1930 that studies on hypotensive agents were subject to the placebo effect, the vast majority of articles and most research, even up to the present day, is conceived, executed, and reported without adequate controls, indicating a failure to appreciate the powerful effects of the placebo. Some of the variables in research on hypotensive agents have been pointed out in recent articles by Schapiro, Myers, Reiser, and Ferris (1954) and Schapiro (1955). During the last twenty years, not only has the literature on the placebo effect been surprisingly meager, but the principles that follow such understanding have not been absorbed into the mainstream of medical research and practice (Hailman, 1953). The placebo is not indexed or discussed in LaWall's *Four Thousand Years of Pharmacy* (1927) or, as pointed out by Pepper, in modern books on treatment. An issue of the *Memphis Medical Journal* (1941) is devoted to an evaluation of drugs, which included ten articles on the use and abuse of cathartics, vitamins, hormones and other drugs. The word "placebo" is not mentioned though some of the concepts implicit in an understanding of the placebo effect are discussed.

In 1938 one hundred million dollars was spent by the American public for vitamin preparations. The total amount spent on medical care in the same year was approximately one billion dollars (Sanford, 1941). Thus, approximately 10 per cent of the nation's medical expenses was spent for vitamins, which are so often prescribed both knowingly and unknowingly for their placebo effect (*J.A.M.A.*, 1959). Dunlap, Henderson, and Inch (1952) analyzed over 17,000 prescriptions of physicians from representative areas in Great Britain for a one month period. Approximately one third were considered to be in the placebo category. The *British Medical Journal* (1952) editorialized that, ". . . a bottle of medicine

is given as a placebo in about 40 per cent" of the patients seen in general practice, a figure that is close to Cabot's (1906) estimate of 44 per cent of the prescriptions filled by Boston Back Bay drug stores.

Recently the influence of size, color, and taste on the effectiveness of placebos has been discussed, although it should be remembered, that "What constitutes a good placebo cannot be specifically stated, because what may be good for one patient may not be so for another even though he has a similar condition" (*J.A.M.A.*, 1955).

Leslie (1954) discusses the influence of color on the effectiveness of placebos, advising red, yellow, or brown rather than blue or green which are associated with "poisonous or external-use-only liquids." The use of red coloring agents, such as alkaline aromatic solution, has long been standard prescription practice, and officially sanctioned by the United States Pharmacopoeia and the National Formulary. He feels that size is also important, tiny and oversize tablets and capsules being much more impressive than average sized ones, ". . . the tiny ones suggesting great strength and the jumbo ones impressing by its heroic size." The more a tablet resembles aspirin the less effective the psychological effect. It is probable that prescribing nine or eleven drops rather than the usual ten would add to the effectiveness of the placebo effect. Taste has always been important. The aromatic elixir (Wolff, 1946), the compound tincture of gentian (Dubois, 1946; Findley, 1953; Leslie, 1954; Wolff, 1946), and asofetida were standard bitter placebo medications used in the past. A more contemporary bitter solution can be prepared with a 0.1 per cent solution of sodium dehydrocholate. Wolff (1946) finds the ammoniated tincture of valerian to be a very efficacious placebo because of "the riot of tastes and smells in the preparation." It may be pointed out that the simple lactose tablet has fallen into disuse because it can be easily recognized and because it can be tasted and the prescription easily read and interpreted. How much more effective is a strange-tasting drug with a complicated and esoteric appellation?

The gastro-intestinal tract, from one end to the other, has been especially prone to exploitation by the placebo. It would be superfluous at this time to name the hundreds of placebo medications that have been used and continue to be used in the treatment of dyspepsia, gastric distress, irritable bowel, bulemia, constipation, and even for conditions such as peptic ulcer. Purgatives and enemas were known and used prior to Hippocrates. Louis XIII was so enthusiastic about their usefulness that he prescribed them for his hunting dogs. Voltaire advised young men to secure a wife who could give an enema pleasantly and quickly (Seager, 1941). When one considers the limited advantages and probable harmful consequences of emptying the intestinal tract, such as, dehydration, exhaustion, discomfort, loss of nutrition, disturbance of sleep and even possibly death (the mortality from appendectomy being 10 per cent greater if a purgative is taken prior to operation), it is difficult to understand the continued use of these substances (Campbell, 1941). Advertisements for colonic irrigations are seen frequently in most daily newspapers. The expenditure for proprietary drugs of this sort must reach millions of dollars.

The odor of tincture of benzoin makes the inhalation of steam so much more acceptable to patients. The effectiveness of placebos on allergic rhinitis, hay fever, and other nasal conditions is well-known. Headaches respond flagrantly to placebo medication. The effectiveness of Epsom salts used in wet dressings for inflamed areas is not due to the magnesium sulfate which is the active ingredient, because this has no specific cutaneous effect. This is also true of the methyl salicylate used in numerous preparations for sore muscles. Both are effective by virtue of their placebo effect (Leslie, 1954; Hyman, 1946).

The so-called tonics in medicine are notorious placebos. The most active part of the tincture of condurango, a bitter tonic and stomachic, and the fluid extract of cimicifuga nigra, a tonic and antispasmodic, is in their impressive names. Tonics of more recent vintage are the elixir of iron, quinine and strychnine (Findley, 1953; Gold, 1946). Some of the most widely used drugs today are prescribed both knowingly and un-

knowingly for their tonic or placebo action. Those drugs that are given in inappropriate circumstances or inadequate dosages can be designated as placebo medication. These would include all tonics and most of the vitamin, liver, hormone preparations and a host of proprietary medications, drugs which stimulate and sedate the patient, control and stimulate appetite, the many combinations of drugs with opposing action (the "if-in-doubt" medication) which have recently appeared, and even many potent drugs.

Placebo effect since 1945

The placebo effect is not a phenomenon limited to the past. Dubois (1946) points out that, "You cannot write a prescription without the element of the placebo. A prayer to Jupiter starts the prescription, and it carries the weight of two or three thousand years of medicine" (Findley, 1953; Houston, 1938; Gold, 1946; *Lancet*, 1954; Leslie, 1954; Modell, 1955; Whitehorn, 1958; Wolff, 1946). The prescription is written by the doctor, and although the Latin of the past is disappearing, the polysyllabic names remain. It is then taken to special stores to be filled by specially trained men and is frequently expensive. Any treatment procedure, including all medication, even potent and pharmacologically effective medicines, have potential placebo effects inherent within them (Dubois, 1946; Findley, 1953; Houston, 1938; Gold, 1946; Hyman, 1946; *Lancet*, 1954; Leslie, 1954; Modell, 1955; Pepper, 1945; Rivers, 1927; Whitehorn, 1958; Wolff, 1946).

Let us trace the further development of the word itself.

Dubois (1946) lists three classes of placebo. The first is the pure placebo, such as lactose, which has no possible intrinsic action. The second is the impure or adulterated placebo which contains some active ingredient but which has no effect on the patient's disease. The third is not a placebo itself but is the psychotherapeutic element inseparable from any medication.

Gold (1946, 1954) (who was either responsible for or one of the earliest to use the term "blind-test" for what later became the "double blind procedure," and who has been a major influence in bringing the placebo to the attention of the medical profession) and Clark (1946) view the placebo as a chemical device for psychotherapy.

Gaddum (1954) feels that the placebo is defined as a medicine given more to please than to benefit the patient, and as such, is given to have an effect. He proposed the use of the "dummy tablet" instead, as a form of treatment which is intended to have no effect, which does not aid therapeutic suggestion or work through a psychological mechanism. Beecher (1956 a) differs with Gaddum, feeling that although the word lacks precision it is adequate. Gaddum's idea has not been absorbed into the literature to any extent.

Leslie (1954) widens the definition: "A placebo is a medicine or preparation which has no inherent pertinent pharmacologic activity but which is effective only by virtue of the factor of suggestion attendant upon its administration. The substance may be ingested, injected, inserted, inhaled or applied." Leslie thus adds preparations that are applied to the list of placebos. He also offers a mechanism, suggestion, as part of his definition.

Beecher summarizes the purposes and uses of placebos, "... as a psychological instrument in the therapy of certain ailments arising out of mental illness, as a resource of the harassed doctor in dealing with the neurotic patient, to determine the true effect of drugs apart from suggestion in experimental work, as a device for elimination of bias not only on the part of the patient but also, when used as an unknown, of the observer, and finally, as a tool of importance in the study of the mechanism of drug action."

Findley (1953), quoting Houston, divides placebos into three types: 1. The type of drug which the physician knows to be inert, but which the patient believes to be potent. 2. Both the patient and the physician believe the drug to be potent, but which is really inert. 3. The patient and the physician both believe a drug to be helpful but which is actually harmful. Findley's concern is with the failure of some physicians to recognize

that they are prescribing placebos, and with the harmful effects which may result.

Despite the aforementioned, the usual definition of the placebo, though with some variation, is similar to that appearing in Dorland's *American Illustrated Dictionary* (1951), where it is defined: "An inactive substance or preparation formerly given to please or gratify a patient, now also used in controlled studies to determine the efficacy of drugs" (Blackeston's, 1956; Oxford, 1933; Shorter Oxford, 1944; Skinner, 1949; Webster's, 1956). This definition is interesting in that it harbors a projective mechanism. Patients are not pleased or gratified by the taking of placebos. It is the doctor who invests the giving of placebos and those patients responding to placebos with a variety of feelings which involve guilt and/or hostility, and which are then projected onto the patient and rationalized as the patient being pleased or gratified. An improved definition of the placebo appears in English and English (1958): "A preparation containing no medicine (or no medicine related to the complaint) and administered to cause the patient to believe he is receiving treatment." However, this definition is inadequate also because it fails to mention that anything offered with therapeutic intent may be a placebo, whether it be given with or without the conscious (or unconscious) knowledge of the doctor or therapist.

Although the placebo is old in history, its use in controlled studies is new and appears frequently in contemporary definitions (*Brit. Med. J.*, 1952; Dorland, 1951; *J.A.M.A.*, 1955; Skinner, 1949).

Beecher, Wolf and Lasagna are of considerable importance to the history of the placebo effect. Beecher (1952; 1955; 1956 a; 1956 b; 1957; Beecher, Keats, Mosteller, & Lasagna, 1953) believes the effectiveness of placebos to be related to the degree of stress or anxiety, the placebo modifying the subjective response to an original sensation or by effecting the reaction component of stress. Wolf's (1950) classic experiments with "Tom" which demonstrated how placebos could reverse the pharmacologic action of drugs and cause end organ changes was an early experimental stimulus to the study of the placebo effect. He showed how placebos could cause extensive "toxic" reactions (Wolf and Pinsky, 1954), and experimentally explored other attributes of placebos (Abbot, Mark, & Wolf, 1952; Hagans, Doering, Ashley, & Wolf, 1957; Wolf, Doering, Clark, & Hagans, 1957). Lasagna has contributed important papers (Beecher et al., 1953; Lasagna, Mosteller, von Felsinger, & Beecher, 1954; von Felsinger, Lasagna, & Beecher, 1955; Lasagna, 1956).

The increased interest in the placebo effect is evident from the data in Figure 1 and Table 1. Articles on the placebo effect have even begun to appear in commercial pharmaceutical journals (*Physician's Bull.*, 1955a, 1955b; *Spectrum*, 1957a). Methodological problems (and the placebo effect indirectly) are explored with increasing frequency in medical journals. Books now contain full chapters on the placebo (Modell, 1955; Hollander, 1958).

The belated interest in the placebo effect

Many authors have commented on the surprising belated interest in the placebo effect. Its potency, widespread use, and therapeutic potentialities have not been acknowledged until recently. Study of its underlying mechanism and dynamics have begun to appear relatively late in the history of medicine.

Houston (1938) wonders how the placebo effect, which is so omnipresent, could have escaped scientific notice for so long a period of time. Pepper (1945) points to the resistance against recognition of the placebo effect that has existed throughout the ages, including the period up to 1945. Dubois (1946) states, ". . . the study of the placebo is the most important step to be taken in scientific therapy." He continues, ". . . although placebos are scarcely mentioned in the literature, they are administered more than any other group of drugs . . . that although few doctors admit that they give placebos, there is a placebo ingredient in practically every prescription . . . that the placebo is a potent agent and its actions can resemble almost any drug." Findley (1953) states that the placebo is the most important

therapeutic weapon in the hands of even the modern physician. Leslie (1954) is surprised, "... that in the light of their importance and everyday use placebos have until so recently been all but neglected in medical curricula and writings." Lasagna (1956) says, "Considering how profound an effect placebos have ... it is surprising how little study has been given to identifying the characteristics of placebo reactors." Wayne (1956) comments on the little interest that has been taken on the action of placebos. *Lancet* (1954) editorialized, "Our predecessors paid little attention to such a lowly menial, and published references to it were few. But the spread of scientific methods to the study of materia medica has led to a remarkable improvement in the status of ... the placebo which until recently ... has never been regarded as quite respectable."

The situation has changed since 1952 as can be noted from Figure 1. More than seven articles on the placebo have appeared each year since 1953. The word "placebo" itself appears more frequently in articles on the placebo effect specifically, and in titles of articles of all types (Table 1).

Factors responsible for the belated interest in the placebo effect

There are several possible factors for the belated interest in the placebo effect.

The first possibility is that there is a preconscious (or unconscious) perception of the placebo effect which is not verbalized or acknowledged because its revelation would be anxiety provoking or threatening. This is based on the premise that a consideration of the placebo effect has long been obvious and overdue. The energy required for this suppression (or repression) is derived from anxiety related to loss of self esteem and guilt. Placebos are often given to demanding and difficult patients, when the physician is frustrated personally and professionally, when the physician's knowledge, or medical knowledge in general, is inadequate to the presenting problem, when the doctor-patient relationship is deteriorating, when the physician is in a hostile, punitive or disdainful frame of mind, when too busy to do anything else and when there is nothing else to do because of failure to respond to the usual therapeutic attempts. Physicians often prescribe with the full knowledge, or at least the suspicion, that these substances are placebos, although communicating to the patient, by omission, or commission, that these inert substances are active. The resultant defensiveness and feelings of guilt would add to the difficulty of discussing the placebo with equanimity and carry over to the placebo effect also (Clyne, 1953; Hofling, 1955; Whitehorn, 1958). It may be threatening for some to give up therapeutic methods, abilities or beliefs which required many years to develop and acquire. Others may be threatened by a real or imagined loss of self esteem and prestige among colleagues and in the community and by the threat of uncertainty or loss of magical powers. [Parsons (1951) speaks of the latter in positive terms. He believes that "optimistic biases" are very general and fundamental in human social organization, and are responsible for the pseudo belief in the efficacy of measures, and the magical beliefs and practices that cluster about situations wherein there is an important uncertainty factor and strong emotional interests in the force of action.]

Another factor may be lack of knowledge or rigidity in thinking. If there is no understanding that there is a phenomenon such as the placebo effect, and that all medication has a placebo effect in part, the concept cannot be considered. The consequences are as harmful today as they were in medicine prior to the seventeenth century when it was not known that the effectiveness of almost all medication was due to the placebo effect. A third possibility is an understanding of the placebo effect but a belief that it is unimportant or inadvisable to discuss it. We cannot accept this possibility as plausible, the purpose of this paper being to indicate the converse proposition. The last two factors may be a consequence of the first factor, i.e., the threatening nature of the material making it possible for lack of knowledge, the rigidity in thinking, a belief in the unimportance of and the inadvisability of discussing the placebo.

Factors responsible for the recent interest in the placebo effect

How can we account for the recent increased interest in the placebo effect? Scientific knowledge has continued to increase geometrically. This increase in knowledge includes an understanding of the placebo effect. With the increased number of therapeutically effective drugs there is less of a necessity to rely on drugs with a placebo effect. As a consequence the resistance against acceptance of this phenomenon decreases. Individuals are less threatened and the silence about the placebo effect begins to vanish. Another factor may be the repetitious fanfare heralding the effectiveness of new drugs with which the drug companies are increasingly flooding the market. The vast majority of these preparations are relegated to silent oblivion. Repeated experiences of this nature should influence one's thinking about the placebo effect as a consequence. Public and private financial support of research has increased enormously in recent years. Between 1952 and 1955 alone, there has been a fiscal increase of 77 per cent in government support of life-science research and an increase of 33 per cent in the number of projects (Consolazio and Jeffrey, 1957). Many more individuals are involved in research. The production of scientists, as reflected in the number of doctorates granted each year, has roughly doubled each decade (National Academy of Sciences, 1958). Methodological problems are examined. The placebo effect cannot be ignored.

The influence of psychiatric thinking on the sciences has increased following World War II. Psychologically, psychoanalytically, and socially oriented psychiatrists have entered the medical schools and hospitals in increasing numbers. There is more interdisciplinary contact and research. It should not be surprising that their concern with psychological factors, symbolism, the doctor-patient relationship, transference and counter-transference should influence scientific thinking in the direction of recognizing the extensiveness of psychological phenomena. Psychiatrists have been active in extending their knowledge to the rest of the medical profession and especially to the general practitioner. This is reflected in the numerous lectures, conferences, and books, as well as numerous articles which are directed toward the general practitioner or other medical specialist (Balint, 1955; Clyne, 1953; English, 1930; Fisher, 1956; Hofling, 1955; Levine, 1942; Romano, 1952; Salfield, 1953).

Despite this, it is difficult to document because psychiatrists (and psychologists) have published the least number of articles on the placebo effect, and only in recent years have they been concerned with it at all (See Figure 1 and Table 1).

PSYCHIATRY AND THE PLACEBO EFFECT

Placebo effect as a problem in Psychiatry.

Psychiatry is in greater need of the knowledge and of the principles underlying and stemming from the placebo effect than medicine or the other sciences. Psychiatrists manage patients with psychological problems almost exclusively, and are involved in a much more meaningful and closer doctor-patient relationship, a situation which maximizes the placebo potential.

The need for psychiatry to be concerned with the placebo effect is readily apparent from a review of the literature on almost any aspect of psychiatry. Contradictory reports abound in all areas. Although it is not the purpose of this paper to review the literature on therapy and other aspects of psychiatry, some of the highlights should be mentioned. In the treatment of manic depressive psychosis, Bellak (1952) lists fifty odd methods, nearly each one having "... been reported as leading to recoveries and/or improvements." Some of the methods of treatment were: testicular vein blood serum, "own blood" injections, lysate of the hypophysis, epivector serum, refrigeration, aurora-tone films, puppets, and diencephalic radiation. The history of the treatment in schizophrenia is even more impressive in this respect (Bellak, 1948). Horwitt (1956) points out how the papers reporting the pathology of schizophrenia year after year, repeated by each new generation," ... would make the schizophrenic patient a sorry physical specimen indeed: his liver,

brain, kidney, and circulatory functions are impaired; he is deficient in practically every vitamin; his hormones are out of balance, and his enzymes are askew." One can find support in the literature for every possible viewpoint about psychiatric treatment. Many papers have appeared which question the efficacy or the possible placebo-like component of psychotherapy (Appel, Lhamon, Myers, & Harvey, 1951; Frank, Gliedman, Imber, Nash, & Stone, 1957; Gliedman, Nash, Imber, Stone, & Frank, 1958; Grinker, 1958; Imber, Frank, Nash, Stone, & Gliedman, 1957; Lewis, 1941; Levitt, 1957; Murphy, 1951; Rosenthal and Frank, 1956; Smith and Wittson, 1957; Whitehorn, 1958), electric convulsive treatment (Brill, Crumpton, Eiduson, Grayson, Hellman, Tichards, Strassman, & Unger, 1957; Bond, & Morris, 1954; Karagulla, 1950; Roth, 1952; Zubin, 1953), insulin coma treatment (Ackner and Harris, 1957; Bourne, 1953; Bourne, 1958; Boardman, Lemas, & Markowe, 1956; Boling, Ryan, & Greenblatt, 1957; Fink, Shaw, Gross, & Coleman, 1958; Leyton, 1953) or the other somatotherapies and psychotherapies in general (Appel et al., 1951; Boardman et al., 1956; Bourne 1957; Clyne, 1953; Diethelm, 1936; Hastings, 1957; Laties & Weiss, 1958; Staudt & Zubin, 1957; Tibbetts and Hawkins, 1956). The papers illustrating the unresolved position of the drugs are too numerous to mention. There are between 20 and 30 tranquilizers on the market, each purporting to be effective in the treatment of psychological illness, not to mention variants of the latter, the numerous combinations of the latter with other drugs and the recent rash of stimulant drugs. The tranquilizers are the fastest growing drugs in history, and it was estimated that in 1957 they would move into second place in sales, reaching about forty million prescriptions totalling 175–200 million dollars in retail sales (Plumb, 1957; Bello, 1957).

Will we appear as wise one hundred years from now as do the compilers of the Paris Pharmacologia of a century ago who said, "What pledge can be afforded that the boasted remedies of the present day will not, like their predecessors, fall into disrepute, and in their turn serve only as a humiliating memorial of the credulity and infatuation of the physicians who recommended and prescribed them?" (LaWall, 1927). Perhaps recent psychiatric developments in the history of the placebo will gainsay us a place in history as prescient as the Paris compilers of a century ago.

The belated interest of psychiatrists in the placebo effect

Before outlining the contributions of psychiatrists and psychologists, it should be noted that the latter have been the last to become interested in the placebo effect. Without going into the details of Figure 1, there are only 2 papers by Americans which meet the criteria of being written by psychiatrists, with placebo in the title and appearing in American psychiatric journals. A total of 6 indexed papers with placebo in the title by psychiatrists (Table 1) contrasts with the 36 papers by non-psychiatric physicians.

Although one could object that part of the discrepancy can be accounted for by the comparative preponderance of non-psychiatric investigators this cannot account for the greater part of the discrepancy. One would expect the placebo effect to be more the concern of psychiatrists than of other investigators, and reflect itself in articles on the placebo effect and perhaps in articles with the word placebo in the title. By analogy, one does expect and does in fact find more articles on schizophrenia by psychiatric authors than by others.

This situation is again reflected in the most widely known book on the history of psychiatry. Zilboorg (1941) does not mention the placebo or the placebo effect in his *History of Medical Psychology*. There is no discussion of suggestibility which at times is equated with the placebo effect. There is only a very brief and inadequate discussion of the doctor-patient relationship from this viewpoint. This is true also of Lewis (1941) *A Short History of Psychiatric Achievement*. Some general histories of medicine do slightly better in this respect although they too are inadequate for the most part. Bromberg

(1954) acknowledges the importance and effectiveness of faith in the magico-religious psychotherapy of the past in his history of psychotherapy, *Man Above Humanity*. He does not discuss, however, faith, suggestibility, or the placebo effect as it applies to medication prescribed in the past or present or to contemporaneous treatment in psychiatry. Parsons (1951) is far more at home with "placebo-like" phenomena in the therapeutic process (although he does not use the term specifically) than the aforementioned authors. He discusses medical practice by way of illustrating a theory of social structure in *The Social System*. Perhaps, because he is a sociologist looking at the therapeutic process from the outside, he can be so much more cognizant of these factors than psychiatrists who are a part of the very process they are observing.

Prior to pursuing this study it was thought that psychologists, by virtue of their careful concern with variables in experimentation, would be cognizant of the placebo effect as an important variable. The converse was found to be true, as indicated by the sparsity of articles published by psychologists (Figure 1) and by the few articles appearing in "Psychological Abstracts" (Table 1). This was further confirmed by the impression that the double blind procedure, which implies an understanding of the placebo effect and which is increasingly being used in clinical studies, is virtually an unknown and unused concept among experimental psychologists who are among the most careful of investigators. Many studies, especially those about which there is some contention, would benefit from a re-evaluation with attention given to this variable.

Psychiatric surprise at the scarcity of articles on the placebo effect

A common feature among psychiatric authors who have written about the placebo effect in recent years is the manifestation of surprise at the scarcity of articles on this subject. It implies that the placebo effect was known but that nobody did anything about it. In another way, it highlights psychiatric belatedness in this area.

Hofling (1955) says, "It is a matter of some curiosity when a group of therapeutic agents in widespread use over a long period of time is largely ignored in medical literature. That this has been the case with placebos is a matter of easy verification. A check of the most widely used textbooks of internal medicine, pharmacology, and therapeutics reveals scarcely any reference to the subject, with nowhere so much as a page devoted to its general discussion."

Fisher (1956) says, although, "Placebo therapy is unique in its timeless potency and popularity . . . it has not achieved full scientific status. . . . Placebo therapy, the most common of all therapies, by paradox has a scanty literature with but few references. . . . The subject receives scant notice in textbooks on treatment, and is often ignored in lectures and courses on therapeutics."

Gliedman et al. (1957) comments, "In recent years, the results achieved by placebo have acquired a limited aura of respectability. The ability of inert compounds . . . to modify a variety of conditions . . . has begun to have a serious impact on medical thinking."

Trouton (1957) says, "Although numerous instances of the surprising efficacy of placebos in a wide variety of conditions have been reported lately, little has yet been done towards providing an explanation of the occurrence of placebo reactions, in terms of psychology." Kurland (1957) comments on the ". . . relatively unexplored areas" of the placebo effect and the paucity of information about this subject. [See also Charcot (1893), Freud (1948, 1950a), Janet (1924, 1925) and Jones (1923a) for similar thoughts on "suggestion."]

Recent interest in the placebo effect appears in psychology. Garner (1958) notes the ". . . seldom reported effects . . ." of the placebo, and says, "Much time and energy would be saved if research workers in this area were familiar with the many points of these papers before they undertook to explain the effect of drugs on patient behavior."

Factors responsible for recent psychiatric interest in the placebo effect

How can this situation be explained? The factors mentioned previously—being threat-

ened, lack of knowledge, rigidity in thinking and disbelief in its importance—can be cited again. In addition, the closer relationship between psychiatrists and patients, with all the implications and consequences of transference and counter - transference phenomena, would make it more difficult for the latter to objectively evaluate such phenomena.

It is readily observed that the situation has changed in recent years. (Table 1, Figure 1). In addition to the factors mentioned previously — increased scientific knowledge, greater numbers of research personnel, concern with methodology, increased numbers of psychiatrists in research, etc.—several deserve special emphasis. In the last twenty-five years more therapeutic methods have been introduced into psychiatry than ever previously. Many of these methods—psychotherapy, ICT, ECT, rehabilitative (milieu) efforts, lobotomy and drugs—*appear* to be effective in as yet some unelucidated way. This should result in psychiatry being more secure and less threatened, and being able more readily to face scientific problems and criticize ineffective methods more objectively. The evaluation of these therapies raises many other problems. The complexity of evaluating contradictory reports, the difficulty of comparing different investigative methods, and the problem of isolating complex variables, inevitably leads, from many directions, to a consideration of the placebo and the placebo effect. It is perhaps not fortuitous that psychiatrists have become interested in the placebo effect following the introduction of the tranquilizing drugs in 1954. (Figure 1).

"Psychiatric" contributions to the study of the placebo effect

Despite psychiatrists' relatively recent concern with the placebo effect, they have contributed perhaps more substantially than all other previous groups of investigators. They have applied their special training and understanding with considerable fruitfulness.

Discussion of the placebo effect during the nineteenth century revolved around the concept of suggestion. This was stimulated by the advent of magnetic and hypnotic treatment. This development was not sudden and had historical roots in the past. As pointed out by Janet (1924, 1925), there was a long period of "Miraculous Healing" in which cure was attributed to Gods. A "Metaphysical Stage" followed in which power was invested in a particular person and exemplified by Mesmerism and Christian Science. The "Scientific Stage" (Bernheim, 1889; Janet, 1924, 1925) followed with the advent of hypnotism. These stages stamped their character on discussions of what is now called the placebo effect.

In the latter half of the 19th century many papers appeared on suggestion theory and therapy with particular emphasis on hypnosis. Hypnosis was viewed as the scientific attempt to investigate and use the principles of psychotherapy which had acted so blindly in the past (Bernheim, 1889; Janet, 1924, 1925). Suggestion was equated with hypnosis and hysteria and most of the papers used these terms as if they were synonymous (Bernheim, 1889; Breuer and Freud, 1957; Charcot, 1925; Ferenczi, 1950; Janet, 1924, 1925; Jones, 1923a, 1923b; Stekel, 1923). Although suggestion and hypnosis are related to the placebo effect and have many common principles, characteristics, and implications for each other, there is evidence (which will not be gone into at the present time) that these phenomena differ from one another in many details (e.g., conversion hysterics are poor placebo reactors, placebo effects can occur without direct, unconscious or conscious suggestions, etc.). For this reason, papers on hypnosis and suggestion per se, unless they have some importance for an understanding of the placebo effect, will not be summarized here (and are readily available elsewhere: Bernheim, 1889; Hull, 1933; Janet, 1925; Schilder, 1956; Weitzenhoffer, 1953).

Bernheim (1889) demonstrated with many appropriate examples the "suggestive" factors which led to ". . . the possible efficacy of various practices employed for the cure of disease and which have succeeded each other from ancient to modern times." His understanding of this phenomenon is based on his concept of hypnosis which is thought to be

merely an extension or heightening of suggestion operating or present in every individual and involving all of human activity.

Similarly, Janet (1924, 1925), although critical of Bernheim's formulations about suggestibility and hypnosis, is well aware of the "suggestive" factors in the history of medical treatment. Janet criticizes the methodology of those attributing cures to miraculous, magical, religious and other suggestive forms of treatment. He cites the inadequacy of the diagnosis, records and statistical treatment, the poor definition of cure, evidence for cure and verification of the cures, and touches on the failure to formulate specificity, laws and indications for treatment. However, he is well aware that there is enough valid evidence for considering these treatments as truly effective and significant. He discusses the factors in the patient, physician and situation which are thought to underlie these successes: In the patient—enthusiasm, faith, belief, feelings in general, power of the imagination, expectant attention, faith in authority, the importance to the patient of being the object of investigation; in the physician—undoubting enthusiasm, faith, and belief, the unconscious personality of the healer; in the situation—the ritual, mystery and strangeness of the proceedings and the situation, changed environment, repetitive education, and suggestibility factors.

Freud (1950a) is in essential agreement with Janet that the majority of primitive treatments "must be classed under the head of psychotherapy," that this element enters into every therapeutic process, that all physicians use it, that it should be studied rather than used blindly, and that "a condition of expectant faith" was induced to effect cures. Concerning drugs Freud says, "It is not a modern dictum but an old saying of physicians that these diseases (psychoneuroses) are not cured by the drug but by the physician, that is by the personality of the physician, inasmuch as through it he exerts a mental influence." It is of interest that Freud viewed treatment in "hydropathic establishments" as effective, whereas today we would class such a cure as due to the placebo effect. Later, in 1912 and 1913, Freud (1949; 1950b; 1950c) emphasized the concept of transference, and made it the cornerstone for explaining the success of suggestive cures. Still later Freud (1948) says "Now that I once more approach the riddle of suggestion after having kept away from it for some 30 years, I find there is no change in the situation," and that it is necessary to formulate a concept of suggestion correctly and limit its extended use. He goes on, "But there has been no explanation of the nature of suggestion, that is, the conditions under which influence without logical foundation takes place."

Ferenczi, in 1909 (1950), holds that suggestion, hypnotism and healing of mental conflicts occurs "through repression, displacement and transference of disagreeable complexes," particularly transference of "repressed parental complexes" and of "unconscious libidinal impulses" onto the person of the suggestor, hypnotist, healer and physician.

Jones (1913; 1923a; 1923b) differentiates between two processes in suggestion—verbal and affective suggestion. The latter is more important and the basis for the former. "Affective suggestion" is the basis of therapeutic suggestion, and is "a rapport which depends on the transference of certain positive affective processes in the unconscious . . . " which are ". . . always components or derivatives of the psychosexual group of activities . . . one form of the more general mechanism of displacement, by means of which an affect is transferred from an original, unpleasant and repressed conception to another less acceptable one. . . . It acts by releasing the repressed desires that are finding expression in the form of symptoms, and allowing them to become attached to the idea of the physician, psychologically this means replacement of one symptom by another. . . ."

Hollingsworth (1912), a psychologist, following the lead of Rivers (1908), published a monograph on *The Influence of Caffein on Mental and Motor Efficiency*. Rivers is quoted approvingly and most of the latter's techniques are incorporated into the study.

Glover's (1931) paper on *The Therapeutic Effect of Inexact Interpretation*, although it

does not discuss or mention the placebo, is concerned with the factors operating in successful suggestion from a psychoanalytic viewpoint and has many implications for an understanding of the placebo effect. The principle method is thought to be the increased effectiveness of repression, backed by strong transference authority, the specific technique depending on the emphasis placed on various defensive mechanisms.

An excellent (practical and theoretical) discussion of "suggestion therapy" (which includes the placebo as one type of such therapy) appears in Levine's (1942) *Psychiatry and Medical Practice*. He discusses the ethics, indications, contraindications, dangers and theoretical factors from a psychoanalytic view. Diethelm (1936) briefly discusses the ethics of using placebos and comments on the suggestive influence of certain psychiatric treatments. As one of the participants in the 1946 Cornell Conference on Therapy, Diethelm (1946) comments on the little progress in understanding suggestibility (which includes the placebo effect) and stresses the belief on the part of the patient and the physician, and the physician's symbolic value and personality as important factors influencing suggestibility. At this same conference Wolff (1946) relates the placebo effect to his experiments on pain thresholds and to the pill being a potent symbol of the doctor which supports the patient in the former's absence. It symbolizes being cared for and gives emotional support or endorsement. It satisfies needs to feel dependent or may fulfill a need to be punished, and supports the desire to get well.

Appel et al. (1951) tabulate the results of psychotherapy with the neuroses and psychoses. They find it striking ". . . . that individual reports from various sources show little variation from the average improvement rate of 67 per cent in the psychoneuroses and 39 per cent in the schizophrenic illnesses," despite a wide variety of procedures which included reassurance at the one end of a continuum and psychoanalysis at the other end of the continuum. They discuss many of the variables that might have determined these results, and raise the question about the usefulness of psychotherapy which has not been adequately tested up to the present time. The paper does not mention the placebo effect specifically but considers many of its facets which would be called the placebo effect by other authors.

Hofling (1955), in an insightful article on the placebo effect for general practitioners, widens the concept of the placebo by defining it as, "A therapeutic measure which is administered largely or entirely for a psychologic effect not inherent in its physical or pharmacologic properties." He goes on to discuss the factors influencing the effectiveness, the therapeutic indications and the ethics of placebos. His elucidation of the attitudes and feelings ("counter-transference phenomena") that accompany, result in or result from the prescribing of placebos is of considerable interest and a major contribution to the understanding of the placebo effect. These factors, rarely considered previously, are shown to influence the effectiveness of and the attitudes toward the placebo. This aspect has been the special and unique contribution of psychiatry and has been considered by others in addition (Clyne, 1953; Whitehorn, 1958).

Abramson, Jarvik, Levine, Kaufman, and Hirsch (1955) discuss the placebo effect as related to LSD, enumerate important variables in such studies, list some of the characteristics of placebo reactors, and correlate the placebo effect with responses to the Cornell Medical Index Health Questionnaire, arithmetic test, Rorschach, and the Wechsler-Bellevue Intelligence Scale.

Fisher and Olin (1956) in a clinical study of patients receiving medication with potent autonomic properties (including a placebo in a double blind study), discuss the dynamics of placebo therapy. A broad definition of the placebo is offered, "A placebo may be any object offered with therapeutic intent," and adds that, "The 'placebo reaction' occurs in psychotherapy even without a pill, in the so-called 'transference' cure." Traits of the positive and negative placebo reactor stemming from this study are delineated.

Tibbetts and Hawkins (1956) cite their experiments with physical treatments in psychiatry and relate their findings to the placebo effect. Traits found to characterize the positive placebo reactor are listed. Baker and Thorpe (1957) conducted a clinical study in which a placebo control resulted in significantly greater improvement than a tranquilizer. They list several of the methodological variables of importance in understanding the placebo effect which were felt to be responsible for this astonishing outcome. Trouton (1957) reviews the literature and relates the placebo effect to learning and personality theories. Hoffer (1956) comments on " . . . . the psychologic or placebo factors that no longer can be ignored." He lists three psychological factors that must be considered when drugs are studied, and cites his experiences with LSD in this regard. Psychodynamic and conditional reflex actions of placebos are discussed by Kurland (1957) who offers some tentative conclusions resulting from a study now in progress.

A number of excellent papers on the placebo effect have emanated from a group at the Johns Hopkins Medical School. Psychotherapy and the placebo effect is discussed by Rosenthal and Frank (1956). They review the literature and demonstrate the importance of considering this factor in research and evaluation of psychotherapy. Gliedman et al. (1957) summarize some conditional research studies on dogs and relate placebo reactivity to established learning concepts. Gliedman et al. (1958) elaborate further on the placebo effect in the context of a study comparing short-term psychotherapy with a placebo. Both procedures resulted in similar "symptom reduction" which was not significantly different from one another. They relate the nature of the symptoms relieved to Beecher's concept of the "reaction component of suffering." Although they comment on the limitations of their study, and many others could be raised, it does represent an attempt to study experimentally the effect of the placebo in the very complicated and difficult area of psychotherapy. Whitehorn, in a recent article (1958), defines the placebo effect as, " . . . . all those psychological and psychophysiological benefits or determinants which quite directly involve the patient's expectations and depend directly upon the diminution or augmentation of the patient's apprehension by the symbolism of medication or the symbolic implications of the physician's behavior and attitudes." This definition adds a dynamic dimension to the usual definition—the expectation and apprehension of the patient. This succinct and insightful article touches on most of the major questions about the placebo effect—theory, psychotherapy, ethics, double blind studies, the doctor-patient relationship, psychodynamics, and the importance of research on this subject. (See also Frank et al., 1957; Imber et al., 1957).

Many of the above articles review the literature on the placebo. Substantial contributions to methodological problems, which involve the use of placebos and indirect concern with the placebo effect, are appearing in increasing numbers by psychiatrists (Feldman, 1956; Horwitt, 1956; Sabshin and Ramot, 1956; Sainz, Biegelow, & Barwise, 1957; Smith & Wittson, 1957; Rashkis & Smarr, 1957a; 1957b).

In 1956 a three day "Conference on the Evaluation of Pharmacotherapy in Mental Illness" was sponsored by the National Academy of Sciences (1956) and should be published in the near future.

DISCUSSION

The placebo effect appears to consist of three factors which are distinct from one another but which are often used interchangeably in the literature.

The first involves methodological errors resulting from variables other than the experimental variable determining results. These errors can arise at any and all stages of an experiment. If the experimental group is not compared with a control group which has been matched, randomized or analyzed for age, sex, acuteness or chronicity of illness, length of hospitalization, diagnosis, prognosis, psychodynamic factors, nutritional and physiological states and a great number of other variables, the experimental results are very likely to be erroneous.

These controls aid in minimizing the omnipresent positive or negative bias of investigators and observors. The effect of spontaneous remission during the course of illness must be considered in the evaluation of therapeutic efficacy. The increasing use of the double blind procedure (Shapiro, Dussik, Tolentino, & Azekoff, 1959) reflects the concern of investigators with these problems.

Studies planned with diligence in every respect, including the use of the double blind procedure, can result in a placebo appearing significantly superior to a tranquilizing drug if the placebo is sweet and the tranquilizer bitter; and if the placebo can be dissolved or easily distinguished from the active drug, the results may reflect, not the effect of medication, but the attitudes of the personnel administering and evaluating the drug (Baker & Thorpe, 1957). The effect of the observer's attitude determining results with tranquilizing drugs has also been demonstrated graphically by Feldman (1956) who found that the reported therapeutic effect of chlorpromazine correlated closely with the physician's attitude to the drug. Many other such examples could be cited.

After examination of the experimental evidence, as well as a consideration of the history of medicine, the literature on the placebo effect, suggestibility, etc., another factor appears which may be a variable but which is different from the usual methodological variables. If all the known and possible variables are controlled or excluded except for this one the results are very likely to be in error. This factor is the placebo effect, the effect of the doctor himself (or some symbolic representative of the doctor) as therapeutic agent (Freud, 1950a; Findley, 1953; Houston, 1938), the most widely used of all agents (Balint, 1955; Cornell Conference, 1946; Fisher & Olin, 1956; Janet, 1924; Jones, 1923a).

This factor assumes special importance because it has dynamic therapeutic effects which are different from simple variables such as age and chronicity of illness. The powerful therapeutic potential of the placebo effect should be further investigated so that its mechanism is understood and its possible uses explored. It may help elucidate some of the problems in psychotherapy and liberate for further inquiry the effects of psychotherapy which appear to be above and beyond the mere result of the placebo effect. The principle(s) underlying the effect of this variable can be extended to non-therapeutic experimental situations which involve an interpersonal relationship with an investigator, or even some symbolic representative of the latter. The effect of the observer or investigator in experimentation requires further exploration.

A third factor is the effect of the placebo when it is an uncontrolled variable in experimental research or therapeutic evaluation which then becomes subject to the methodological errors described previously.

Thus, a clarification of the placebo effect requires that a distinction be made between methodological variables, the placebo effect itself, and the placebo effect as an uncontrolled variable. In addition, as correctly pointed out by Fisher (1956), the placebo should be distinguished from the placebo effect, the placebo being the agent which may or may not result in a placebo effect.

These considerations have important implications for psychiatry. It should stimulate the development of the sensitive methodology required by the complex factors in psychiatry. The perennial misevaluation of new therapies may be avoided or limited. The recent flood of newly introduced therapeutic agents and procedures demands reliable evaluation. It is important to tease out the effects due to specific factors and those due to experimental variables and the placebo effect. It is important to determine whether, to what extent, and why a specific therapy may require a placebo effect to be successful, neither possibly being effective separately but only in combination. As pointed out by Rosenthal and Frank (1956), the effects of psychotherapy may be obscured by under or overevaluation because some patients may respond without therapy, some to any kind, and others only to specific therapy. The effect of the "social milieu" or "psychosocial environment" should be clarified and distinguished from the placebo effect if possible, if it is distinguishable in

fact, and not itself a product of the placebo effect (Greenblatt, Levinson, & Shapiro, 1959).

Occasional records of ancient medicine having stumbled upon a useful drug do occur. The inability, however, to scientifically evaluate the specific usefulness of the drug usually resulted in its loss to future generations. The ancient Chinese gave ground-up "dragon bones" to children with convulsions, the calcium being effective for infantile tetany (Haggard, 1933). Burnt sponge, which contains iodine, was used to treat simple goiter (Haggard, 1933). Withering used his digitalis concoction (Fox-glove) for all cases of dropsey, which was regarded as a primary disease (Garrison, 1921; Major, 1955). The recognition that digitalis had a beneficial effect in only certain types of dropsey, that caused by congestive heart failure, and that it was ineffective in renal disease, vitamin deficiency, hypothyroidism, cirrhosis, adrenal malfunction, neoplasm, etc., led to the former's more effective treatment and to advances in the understanding and treatment of the other conditions. Sydenham in the 17th century is credited with a major contribution to the end of "galenicals" in medicine, and the beginnings of scientific medicine, by demonstrating the specificity of cinchoma bark for fever of malarial origin (Garrison, 1921; Haggard, 1929; Haggard, 1934; Major, 1954; Major, 1955; Pepper, 1945).

Although psychiatry is on a much higher level than medicine of that period there are some rough similarities that do not require elaboration.

This paper is not meant to derogate the ingenuity, inventiveness, sensitivity, and value of the individual clinician. The past, present, and future would be bleak indeed if their creative function were superseded by a mechanistic methodology. Scientific methodology rarely if ever creates ideas in itself but is used to test these ideas and concepts. But it is also necessary to recognize the limitations and dangers inherent in the "common sense" method of the clinician. This has been demonstrated clearly in the case of morphine. After all the centuries it has been used, the dosage arrived at in this country is 50 to 100 per cent greater than the one that gives essentially the maximum of pain relief, as determined by careful methodological evaluation. (Beecher, 1952; Denton and Beecher, 1949; Lasagna and Beecher, 1954; Lasagna et al., 1954; Keats, Beecher, & Mosteller, 1950; Lee, 1942). It does not follow, however, that recognition be denied the astute clinician who first used morphine, or those who rediscovered it in other places and at other times, or those who followed reifying its usefulness. Their importance and function is gainsaid. Our concern is with minimizing the errors of the astute clinician and the hyperbole of the less gifted, and with the adding of clarity, objectivity, reliability and sensitivity to the "common sense" and at times the "uncommon sense" of the clinician. This requires concern with methodological problems in general and the placebo effect as a methodological problem in particular. Finally the recognition of the placebo effect, an important variable in methodology, capable of potent and extensive therapeutic effects, requires special attention.

REFERENCES

Abbot, F. K., Mark, M., & Wolf, S. The action of Banthine on the stomach and duodenum of man with observations on the effects of placebos. *Gastroenterology*, 1952, 20, 249.

Abramson, H. A., Jarvik, M. E., Levine, A., Kaufman, M. R., & Hirsch, M. W. Lysergic Acid Diethylamide (LSD-25): XV, The effects produced by substitution of a tap water placebo. *J. of Psych.*, 1955, 40, 367.

Abramwitz, W. The use of placebos in the local therapy of skin diseases. *N. Y. State J. Med.*, 1948, 48: 11, 1927.

Ackner, M. A., & Harris, A. Insulin treatment of schizophrenia. *Lancet*, 1957, I, 607.

Appel, K. E., Lhamon, W. T., Myers, J. M., & Harvey, W. A. Long term psychotherapy. In *Psychiatric treatment. Proc. Ass. Res. Nerv. Mental Dis.*, N.Y., Baltimore: Williams & Wilkins, 1951.

Ayman, D. An evaluation of therapeutic results in essential hypertension. *J.A.M.A.*, 1930, 95, 246.

Baker, A. A., & Thorpe, J. G. Placebo response. *A.M.A. Arch. Neurol. & Psychiat.*, 1957, 78, 57.

Balint, M. The doctor, his patient, and the illness. *Lancet*, 1955, I, 683.

Balint, M. *The doctor, his patient and the illness.* N.Y.: International Universities Press, 1957.

Beecher, H. K. Experimental pharmacology and measurement of the subjective response. *Science*, 1952, 116, 157.

Beecher, H. K. The powerful placebo. *J.A.M.A.*, 1955, 159, 1602.

Beecher, H. K. The subjective response and the reaction to sensation: the reaction phase as site of effective drug action. *Am. J. Med.*, 1956, 20, 107. (a)

Beecher, H. K. Evidence for increased effectiveness of placebos with increased stress. *Am. J. Physiol.*, 1956, 187, 163. (b)

Beecher, H. K. Measurement of pain. *Pharm. Rev.*, 1957, 9, 59.

Beecher, H. K., Keats, A. S., Mosteller, F., & Lasagna, L. The effectiveness of oral analgesics (morphine, codeine, acetylsalicylic acid) and the problem of placebo "reactors" and "non-reactors." *J. Pharmacol. & Exper. Ther.*, 1953, 109, 393.

Bellak, L. *Dementia praecox*. N.Y.: Grune & Stratton, 1948.

Bellak, L. *Manic-depressive psychosis and allied conditions*. N.Y.: Grune & Stratton, 1952.

Bello, F. The tranquilizer question. *Fortune*, May 1957, 162-188.

Bernheim, H. *Suggestive therapeutics*. New York: G. P. Putnam's Sons, 1889.

Block, S. L. Hippocrates on psychotherapy by the general practitioner. *New Eng. J. Med.*, 1957, 256, 559.

Boardman, R. H., Lemas, J., & Markowe, M. Insulin and Chlorpromazine in schizophrenia. *Lancet*, 1956, II, 487.

Boling, L., Ryan, W., & Greenblatt, M. Insulin treatment of psychotic patients. *Am. J. Psychiat.* 1957, 113, 1009.

Bond, E. D. & Morris, Jr., H. H. III: Manic-depressive reactions. *Am. J. Psychiat.*, 1954, 110, 883.

Bourne, H. The insulin myth. *Lancet*, 1953, II, 964.

Bourne, H. Psychotherapy and tranquilizers. *New Zealand Med. J.*, 1957, LVI, 392.

Bourne, H. Insulin coma in decline. *Am. J. Psychiat.*, 1958, 114, 1015.

Breuer, J., & Freud, A. *Studies on hysteria*. New York: Basic Books, 1957.

Brill, N. G., Crumpton, E., Eiduson, S., Grayson, H. M., Hellman, L. I., Tichards, R. A., Strassman, H. D., & Unger, A. A. Investigation of the therapeutic components and various factors associated with improvement with electroconvulsive treatment; A preliminary report. *Am. J. Psychiat.* 1957, 113, 997.

Brit. Med. J. Editorial—The Bottle of Medicine. 1952, I, 149.

Bromberg, W. *Man above humanity—A history of psychotherapy*. Philadelphia: J. B. Lippincott Co., 1954.

Bromberg, W. An analysis of therapeutic artfulness. *Am. J. Psychiat.*, 1958, 114, 719.

Cabot, R. C. The physician's responsibility for the Nostrum Evil. *J.A.M.A.*, 1906, 47, 982.

Campbell, E. G. Use and abuse of cathartics. *Memphis Med. J.*, 1941, XVI, 192.

Carter, A. B. The placebo: Its use and abuse. *Lancet*, 1953, 11, 823.

Castiglioni, A. *Adventures of the mind*. New York: Alfred A. Knopf, 1946.

Cecil, R. A., & Loeb, R. F. *A textbook of medicine*. Philadelphia: W. B. Saunders & Co., 1951.

Charcot, J. M. La foi qui guérit. *Archives de Neurologie*, 1893, 1, 74. Quoted in: Janet, 1924, 1925.

Charpingnon. *Etudes sur la Médecine animque et vitaliste*. Paris: G. Bailliere, 1864. Translated in: Bernheim, 1891.

Clark, Quoted by Dubois, E. F. Cornell Conference on Therapy. *N. Y. State J. Med.*, 1946, 46: 2, 1718.

Clyne, M. B. Letters to the editor—The placebo. *Lancet*, 1953, II, 939.

Colbatch, J. *A dissertation concerning mistletoe: A most wonderful specific remedy for the cure of convulsive distempers*. London: W. Churchill, 1719.

Consolazio, W. V., & Jeffrey, H. L. Federal support of research in the life of science. *Science*, 1957, 126, 154.

Cornell Conference on Therapy. *N.Y. State J. Med.*, 1946, 46: 2, 1718.

DeMaar, E. W. J., & Pelikan, E. W. The use of placebos in therapy and clinical pharmacology. *Mod. Hosp.*, 1955, 84, 108.

Denton, J. E., & Beecher, H. K. New analgesics. *J.A.M.A.*, 1949, 141, 105.

Diethelm, O. *Treatment in psychiatry*. N.Y.: The MacMillan Co., 1936.

Diethelm, O. Cornell Conference on Therapy. *N.Y. State J. Med.*, 1946, 46: 2, 1718.

Dorland, W. A. N. *The American illustrated medical dictionary*. Philadelphia & London: Saunders, 1951.

Dubois, E. F. Cornell Conference on Therapy. *N.Y. State J. Med.*, 1946, 46: 2, 1718.

Dunlap, D. M., Henderson, T. L. & Inch, R. S. A Survey of 17,301 prescriptions on form E 10. *Brit. Med. J.*, 1952, I, 292.

Elkes, J. & Elkes, C. Effect of Chlorpromazine on the behavior of chronically overactive psychotic patients. *Brit Med. J.*, 1954, 11, 560.

English, O. S. On the necessity of applying psychotherapy exclusively. *Med. Rec.*, 1930, 143, 384.

English, H. B., & English, A. C. *A comprehensive dictionary of psychological and psychoanalytic terms*. New York: Longman's Green & Co., 1958.

Fantus, B. *A text book of prescription writing and pharmacy*. Chicago: Chicago Medical Book Co., 1906.

Fantus, B. *General technique of medication*. Am. Medical Association, 1938.

Feldman, P. E. The personal element in psychiatric research. *Am. J. Psychiat.*, 1956, 113, 52.

Ferenczi, S. Introjection and transference. In

Sex in psychoanalysis. New York: Basic Books, 1950.

Findley, T. The placebo and the physician. *Med. Clin. N. Amer.*, 1953, 37, 1821.

Fink, M., Shaw, R., Gross, G. E., & Coleman, F. S. Comparative study of chlorpromazine and insulin coma in therapy of psychosis. *J.A.M.A.*, 1958, 166, 1846.

Fisher, H. K., & Olin, B. M. The dynamics of placebo therapy: A clinical study. *Am. J. Med. Sc.*, 1956, 232, 504.

Frank, J. D., Gliedman, L. H., Imber, S. B., Nash, E. H., & Stone, A. R. Why patients leave psychotherapy. *A.M.A. Arch. Neurol. & Psychiat.*, 1957, 77, 283.

Freud, S. On psychotherapy. *Collected Papers*. Vol. 1, London: Hogarth Press, 1950. (a)

Freud S. The dynamics of the transference. *Collected Papers*, Vol. II, Hogarth Press, 1950. (b)

Freud S. On beginning treatment. Further recommendations in the technique of psychoanalysis. *Collected Papers*. Vol. II, Hogarth Press, 1950. (c)

Freud S. *Group psychotherapy and the analysis of the ego*. London: Hogarth Press, 1948.

Freud S. *An outline of psychoanalysis*. New York: W. W. Norton & Co., 1949.

Gaddum, J. H. Walter Dixon Memorial Lecture: Clinical pharmacology. *Proc. Roy. Soc. Med.*, 1954, 47.

Garner, A. M. *Annual review of psychology*. California: Annual Reviews, Inc., 1958.

Garrison, F. H. *An introduction to the history of medicine*. Phil: W. B. Saunders Co., 1921.

Glaser, E. M., & Whittow, G. C. Experimental errors in clinical trials. *Clin. Sc.*, 1953, 13, 199.

Gliedman, L. H., Gantt, H., & Teitelbaum, H. A. Some implications of conditional reflex studies for placebo research. *Am. J. Psychiat.*, 1957, 113, 1103.

Gliedman, L. H., Nash, Jr., E. H., Imber, S. D., Stone, A. R., & Frank, J. D. Reduction of symptoms by pharmacologically inert substances and by short-term psychotherapy. *A.M.A. Arch. Neurol. & Psychiat.*, 1958, 79, 345.

Glover, E. The therapeutic effect of inexact interpretation: A contribution to the theory suggestion. *Int'l J. Psychoanalysis*, 1931, 12, 397.

Gold, H. Cornell Conference on Therapy, *N.Y. State J. Med.*, 1946, 46: 2, 1718.

Gold, H. In Cornell Conference on Therapy, How to evaluate a new drug. *Am. J. Med.*, 1954, 17, 722.

Goldwyn, R. W. The king's touch for the king's evil. *Harvard Med. Alumni Bull.*, 1958, 32, 18.

Greenblatt, M., Levinson, D. J., & Shapiro, A. K. Placebo effect, social milieu, and the evaluation of psychiatric therapies. *J. Chr. Dis.*, 1959, 9, 327.

Grinker, R. R., Discussion of article by Bromberg, W.: An analysis of therapeutic artfullness. *Am. J. Psychiat.*, 1958, 114, 725.

Hadfield-Jones, R. F. C. A bottle of medicine from the doctor. *Lancet*, 1953, II, 823.

Hagans, J. A., Doering, C. R., Ashley, F. W., & Wolf, S. The therapeutic experiment. *J. Lab. & Clin. Med.*, 1957, 9, 282.

Haggard, H. W. *Devils, drugs and doctors*. New York: Harper & Bros., 1929.

Haggard, H. W. *Mystery, magic and medicine*. New York: Doubleday Doran & Co., Inc., 1933.

Haggard, H. W. *The doctor in history*. New Haven: Yale Univ. Press., 1934.

Hailman, H. F. The blind placebo in the evaluation of drugs. *J.A.M.A.*, 1953, 151, 1430.

Hastings, E. W. Follow-up results in psychiatric illness. *Am. J. Psychiat.*, 1957, 54, 171.

Hoffer, A. Experimental pharmacodynamics and psychobiology. *J. Clin. & Exp. Psychopath.*, 1956, XVII, 376.

Hofling, C. K. The place of placebos in medical practice. *G.P.*, 1955, XI, 103.

Hollander, M. H. *The psychology of medical practice*. Phil.: W. B. Saunders Co., 1958.

Hollingsworth, H. L. The influence of caffein on mental and motor efficiency. *Arch. Psychol.*, 1912, 22, 1–166.

Holmes, O. W. *Medical essays*, 1842–1882. Cambridge, Mass.: The Riverside Press, 1891.

Horwitt, M. K. Fact and artifact in the biology of schizophrenia. *Science*, 1956, 124, 429.

Houston, W. R. Doctor himself as therapeutic agent. *Ann. Int. Med.*, 1938, 11, 1416.

Hull, C. L. *Hypnosis and suggestibility*. New York: Appleton-Century, 1933.

Hyman, H. T. *An integrated practice of medicine*. Philadelphia: W. B. Saunders & Co., 1946.

Imber, S. D., Frank, F. D., Nash, E. H., Stone, A. R., & Gliedman, L. H. Improvement and amount of therapeutic contact; An alternative to the use of no-treatment controls in psychotherapy. *J. Cons. Psychol.*, 1957, 21, 309.

J.A.M.A. The Iatroergic Pill. 1954, 154, 28.

J.A.M.A. Editorial—Placebos. 1955, 159, 780.

J.A.M.A., Council on Food & Nutrition. Vitamin preparations as dietary supplements and as therapeutic agents. 1959, 1, 41.

Janet, P. *Principles of psychotherapy*. New York: The MacMillan Co., 1924.

Janet, P. *Psychological healing. A historical and clinical study*. Vol. 1, London: George Allen & Unwin Ltd., 1925.

Jones, E. The treatment of the neuroses, including the psychoneuroses. In W. A. White and S. Jelliffe, *The modern treatment of neuroses and mental illness*. Philadelphia and New York: Lea and Febiger, 1913.

Jones, E. The action of suggestion in psychotherapy. In *Papers on psychoanalysis*, London: Bailliere, Tindall and Cox, 1923. (a)

Jones, E. The nature of auto-suggestion. In *Papers on psychoanalysis*, London: Bailliere, Tindall and Cox, 1923. (b)

Karagulla, S. Evaluation of electric convulsion therapy as compared with conservative

methods of treatment in depressive states. *J. Ment. Sci.*, 1950, 96, 1060.

Keats, A. S., Beecher, H. K., & Mosteller, F. C., Measurement of pathological pain in distinction to experimental pain. *J. Appl. Physiol.*, 1950, 3, 35.

Koteen, H. The use of a "double blind" study investigating the clinical merits of a new tranquilizing agent. *Ann. of Int. Med.*, 1957, 47, 978.

Kraines, S. H. *The therapy of the neuroses and psychoses*. Philadelphia: Lea & Febiger, 1948.

Kurland, A. A. The drug placebo—Its psychodynamic and conditional reflex action. *Behavioral Science*, 1957, 2, 101.

Lancet. Editorial, The humble humbug. 1954, II, 321.

Lasagna, L. Placebos. *Sci. Am.*, 1956, 193, 68.

Lasagna, L., & Beecher, H. K. The optimal dose of morphine. *J.A.M.A.*, 1954, 156, 230.

Lasagna, L., Mosteller, F., von Felsinger, J. M., & Beecher, H. K. A study of the placebo response. *Am. J. Med.*, 1954, 16, 770.

Laties, V. G., & Weiss, B. A critical review of the efficacy of meprobamate in the treatment of anxiety. *J. Chr. Dis.*, 1958, 7, 500.

LaWall, C. H. *Four thousand years of pharmacy*. Philadelphia & London: J. B. Lippincott Co., 1927.

Lee, L. E., Jr., Studies of morphine, codeine and their derivatives; XVI, Clinical studies of morphine, Metopen and Desmorphine. *J. Pharmacol.*, 1942, 75, 161.

Lennox, W. G. The centenary of bromides. *New England J. Med.*, 1957, 259, 887.

Leslie, A. Ethics and practice of placebo therapy. *Am. J. Med.*, 1954, XVI, 854.

Levine, M. *Psychotherapy in medical practice*. New York: MacMillan, 1942.

Levitt, E. E. The results of psychotherapy with children: An evaluation. *J. cons. Psychol.*, 1957, 21, 189.

Lewis, N. D. C. *A short history of psychiatric achievement*. New York: Norton, 1941.

Leyton, S. R. Glucose vs. insulin. *Lancet*, 1953, I, 1253.

Maimonides. Quoted in Baruk, H., *Les therapeutiques psychiatriques*. Paris: Presses Universitaires de France, 1957.

Major, R. H. *A history of medicine*. Vol. I. Springfield, Ill.: C. C Thomas, 1954.

Major, R. H. *Classic descriptions of disease*. Springfield, Ill.: C. C Thomas, 1955.

Masserman, J. H. *The practice of dynamic psychiatry*. Philadelphia: W. B. Saunders, 1955.

Memphis Med. J. Dec. 1941, XVI, 12.

Modell, W. *The relief of symptoms*. Philadelphia: W. B. Saunders, 1955.

Montaigne. *The essays of Montaigne*. New York: Modern Library, 1946.

Murphy, W. F. Problems in evaluating the results of psychotherapy. *A. M. A. Arch. Neurol. & Psychiat.*, 1951, 66, 643.

National Academy of Sciences, National Research Council. Conference on the evaluation of pharmacotherapy in mental illness. Washington, D. C., Sept. 19–22, 1956.

National Academy of Sciences, National Research Council: Production of U. S. scientists. *Science*, 1958, 127, 682.

O'Brien, J. R. Is liver a "tonic"? A short study of injecting placebos. *Brit. Med. J.*, 1954, 11, 136.

Osler, W. *The principles and practice of medicine*. New York: Appleton & Co., 1892.

Osler, W. Medicine in the nineteenth century. In *Aequanimatas*. Philadelphia: P. Blakeston's Son & Co., 1905.

Oxford New English Dictionary. New York & Oxford: Oxford Univ. Press, 1933.

Parsons, T. *The social system*. Glencoe, Ill.: The Free Press, 1951.

Pepper, O. H. A note on the placebo. *Am. J. Pharm.*, 1945, 117, 409.

Physician's Bulletin, The placebo—A neglected agent? Part 1. 1955, XX, 3. (a)

Physician's Bulletin, The placebo—A neglected agent? Part 2. 1955, XX, 48. (b)

Platt, R. Two essays on the practice of medicine. *Lancet*, 1947, II, 305.

Plumb, R. K. Tranquilizer sales, in Science in Review. *N.Y. Times*, Sunday, August 25, 1957, p. E9.

Rapport, S. & Wright, H. *Great adventures in medicine*. New York: Dial Press, 1952.

Rashkis, H. A., & Smarr, E. R. Psychopharmacotherapeutic research. *A.M.A. Arch. Neurol. & Psychiat.*, 1957, 77, 202. (a)

Rashkis, H. A., & Smarr, E. R. Drug and milieu effect with chronic schizophrenics. *A.M.A. Arch. Neurol. & Psychiat.*, 1957, 78, 89. (b)

Rivers, W. H. R. *The influence of alcohol and other drugs on fatigue*. London: Edw. Arnold, 1908.

Rivers, W. H. R., *Medicine, magic and religion*. N.Y.: Harcourt Brace & Co., 1927.

Romano, J., *In teaching psychotherapeutic medicine*. Cambridge: Harvard Univ. Press, 1952.

Rosenthal, D., & Frank, J. D. Psychotherapy and the placebo effect. *Psychol. Bull.*, 1956, 55, 294.

Roth, M. A theory of ECT action and its bearing on the biological significance of epilepsy. *J. Ment. Sc.*, 1952, 98, 44.

Sabshin, M., & Ramet, J. Pharmacotherapeutic evaluation and the psychiatric setting. *A.M.A. Arch. Neurol. & Psychiat.*, 1956, 75, 362.

Sadler, W. S. *Practice of psychiatry*. St. Louis: C. V. Mosby Co., 1953.

Sainz, A., Biegelow, N., & Barwise, C. On a methodology for the clinical evaluation of phenopraxic drugs. *Psychiat. Quarterly*, 1957, 31, 10.

Salfield, D. J. Letters to the Editor—The placebo. *Lancet*, 1953, II, 940.

Sanford, C. H. Exploitation of vitamins. *Memphis Med. J.*, 1941, XVI, 186.

Schapiro, A. F., Psychological factors in evalu-

ation of hypertensive drugs. *Psychosomatic Med.*, 1955, XVII, 291.

Schapiro, A. F., Myers, T., Reiser, M. F., & Ferris, E. B. Blood pressure response to Veratrum and doctor. *Psychosomatic Med.*, 1954, XVI, 478.

Schilder, P. *The nature of hypnosis.* New York: International Universities Press, 1956.

Seager, L. D. Use and abuse of drugs. *Memphis Med. J.*, 1941, XVI, 189.

Shapiro, A. K. The placebo effect in the history of medical treatment (Implications for psychiatry). In Press, *Am. J. Psychiat.* (a)

Shapiro, A. K. Attitudes towards the use of placebos in treatment. Submitted for publication. (b)

Shapiro, A. K. Other papers on the placebo effect in preparation. (c)

Shapiro, A., Dussik, K. T., Tolentino, J. C., & Azekoff, M. The feasibility of "browsing" double-blind studies in large state hospitals, and A "browsing" double-blind study of Iproniazid in geriatric patients; Submitted for publication.

Sigerist, H. E. *The great doctors.* New York: Doubleday & Co., 1958.

Skinner, H. A. *The origin of medical terms.* Baltimore: The William & Wilkins Co., 1949.

Smith, J. A., & Wittson, L. L. Evaluation of treatment procedures in psychiatry. *Dis. Nerv. System*, 1957, XVIII, 1.

Spectrum. The Placebo. 1957, 5, 141. (a)

Spectrum. Mistletoe. 1957, 5, 612. (b)

Staudt, V. M., & Zubin, J., A biometric evaluation of the somatotherapies in schizophrenia. *Psychol. Bull.*, 1957, 54, 171.

Stekel, W. *Psychoanalysis & suggestion therapy.* New York: Moffat Yard & Co., 1923.

Thorner, M. W. *Psychiatry in general practice.* Philadelphia: W. B. Saunders Co., 1949.

Tibbetts, R. W., & Hawkins, J. R. The placebo response. *J. of Ment. Sc.*, 1956, 102, 60.

Trouton, D. S. Placebos and their psychological effects. *J. Ment. Sc.*, 1957, 103, 344.

Trousseau. *Dictionaire de Médicine.* Paris: Libraire de la Faculté de Médecina, 1833. Translated in: Bernheim, 1889.

Tuke, H. *Le Corps et l'esprit. Action du moral et l'imagination sur le physique.* Paris: 1886. Translated in: Bernheim, H., 1889.

Van Dyke, H. B. The weapons of panacea. *Scientific Monthly*, 1947, 64, 322.

Volgyesi, F. A. "School for Patients" hypnosis-therapy and psychoprophylaxis. *Brit. J. Med. Hypnotism*, 1954, 5, 8.

von Felsinger, J. M., Lasagna, L., & Beecher, H. K. Drug-induced mood changes in man. 2. Personality and reactions to drugs. *J.A.M.A.*, 1955, 157, 1113.

Wayne, E. J. "Placebos". *Brit. Med. J.*, 1956, 11, 157.

Webster's New International Dictionary of the English Language. Springfield, Mass.: G. ec. Merriam Co., 1940.

Weiss, E., & English, O. S. *Psychomatic medicine.* Philadelphia: W. B. Saunders Co., 1949.

Weitzenhoffer, A. M. *Hypnotism: An objective study in suggestibility.* New York: Wiley, 1953.

Whitehorn, J. C. Psychiatric implications of the "placebo effect." *Am. J. Psychiat.*, 1958, 114, 662.

Wolf, W. Effects of suggestion and conditioning on the action of chemical agents in human subjects—the pharmacology of placebos. *J. Lab. & Clin. Med.*, 1950, 29, 100.

Wolf, S., Doering, C. R., Clark, M. L., & Hagans, J. A. Chance distribution and the placebo "reactor." *J. Lab. & Clin. Med.*, 1957, 49, 837.

Wolf, S., & Pinsky, R. H. Effects of placebo administration and occurrence of toxic reactions. *J.A.M.A.*, 1954, 155, 339.

Wolff, H. G. Cornell Conference on Therapy. *N.Y. State J. Med.*, 1946, 46: 2, 1718.

Zilboorg, G. *A history of medical psychology.* N.Y.: W. W. Norton & Co., 1941.

Zubin, J. Design for the evaluation of therapy. In *Psychiatric treatment, Proc. Ass. Res. Nerv. Mental Dis.* N.Y.: Williams & Wilkins, 1953.

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A tale is told of a man in Paris during the upheaval in 1848, who saw a friend marching after a crowd toward the barricades. Warning him that these could not be held against the troops, that he had better keep away, and asking why he followed those people, he received the reply, 'I must follow them. I am their leader.'

A. LAWRENCE LOWELL, *Conflicts of Principle*

A static explanation argues that something is so because something else is so. For example, an explanation of a state of affairs in terms of an equilibrium is static. A dynamic explanation, on the other hand, argues that something is so because it has become so. The physicist, for example, views the velocity of a falling stone as a result of force having acted (not merely now acting) on the stone. In this article, the author points out the excessive reliance of many social sciences upon static explanations of social arrangements (equilibria, adjustment, etc.) and the comparative neglect of dynamic (historical) analysis. He also develops heuristic criteria for judging the explanatory value of various social variables in dynamic analysis.

EXPLANATION AND COMPARATIVE DYNAMICS IN SOCIAL SCIENCE

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INTRODUCTION

AMONG social scientists there is now an increasing interest in fundamental theory, in *explaining* social facts rather than merely *describing* them. At present the only social sciences possessing a well-developed systematic theory are economics and demography. But there is a clear trend towards greater emphasis on theoretical analysis in social and cultural anthropology, sociology, social psychology, social and cultural history, political science, legal theory, etc. There is also a growing realization of the fact that society is a causal mechanism so closely inter-connected as to require analysis to a large extent in terms of one basic theory common to all social sciences rather than merely in terms of independent theories particular to the various disciplines. The interest of social scientists in recent years has tended to centre upon problems which by their very nature require study by a cooperative effort of specialists in different social sciences, such as the problems of cultural change, economic development, the relation between culture and personality structure, comparative politics, etc.; and experience with interdisciplinary research projects in such fields has made even clearer to what an extent our understanding of social phenomena is restricted by the lack of an integrated explanatory theory of society.

When a social scientist (e.g., a historian) tries to explain the behaviour of a given individual, he will use the social institutions and cultural patterns of the relevant society as explanatory principles without necessarily attempting to explain these institutions and cultural patterns themselves. But at a deeper level of analysis the main task of the social sciences is precisely to explain the institutions and cultural patterns of each society. The purpose of this paper is to propose certain heuristic criteria for explanatory hypotheses and general theory construction at that level of analysis. Of course, heuristic considerations can never save us the trouble of actually setting up specific explanatory hypotheses and testing them against the social facts of the present and the past. But they may help us to devise fruitful hypotheses and to avoid blind alleys if we can make it clearer to ourselves what sort of theory we are after and what kinds of explanations we are looking for.

STATIC AND DYNAMIC MODELS

The most "natural" explanation for a social fact is in terms of some other social variables belonging to the *same* time period. For instance, the most obvious way of explaining the nature of English literature in the Victorian age is to refer to certain characteristics of English society in that period. But a little reflection will show that

this type of explanation cannot be pressed very far. For, English literature of the Victorian age evidently carried forward (or reacted against) the English literary traditions of earlier ages and was also subject to literary influences from abroad. Hence, what contemporaneous English social conditions can explain about Victorian-age English literature are not so much the prevailing *literary patterns* themselves as are rather the *deviations* that occurred from the literary patterns of the previous period (as well as from the foreign models used). That is, what contemporaneous social conditions directly explain are not the relevant social *variables* themselves but are only their *time trends* (time derivatives[1]), i.e. the directions and rates of their change.

Following the terminology used by economists, we shall speak of *static explanation* when a social variable is explained exclusively in terms of variables belonging to the *same time period*; and we shall speak of *dynamic explanation* when at least some of the explanatory variables used belong to an *earlier period* than the variables to be explained. More generally, we shall speak of dynamic explanation also if what we directly try to explain, and/or what we offer as explanation, involve not only the *values* of certain social variables at a given time, but also their *time trends* (time derivatives), i.e. the directions and rates of their change.

Social scientists making use of a static explanation often do not sufficiently realize how restrictive the assumptions are to which they are committed by adopting this type of explanation. A static explanation implies a static model of the social system. That is, it implies the assumption that all social variables relevant to the problem on hand always adjust to one another (and to the variables defining the natural environment) without any considerable time lag. More particularly, if the value of a certain variable Y is to be explained in terms of the contemporaneous values of some other variables $X_1, \cdots, X_n$, then the following assumptions have to be satisfied:

1. At any moment the values of $X_1, \cdots, X_n$ must determine an equilibrium value for Y.
2. Should Y be removed from this equilibrium value by any disturbing force, it must return again without any significant delay[2] to this equilibrium value when the disturbing force ceases to operate.
3. Should there be any change in the values of $X_1, \cdots, X_n$, then Y must move to its new equilibrium value (determined by the new values of the X's) also without any significant delay[3].

Clearly, these are very strong assumptions, in particular 2 and 3. No doubt, there are within the social system subsystems to which static models are applicable at least as crude first approximations. For instance, the analysis of economic and political institutions in terms of static models and in terms of the concepts of economic or political equilibrium (balance of power) does have its usefulness. But the social system as a whole can hardly be regarded as a static mechanism. If nothing else, at least the evident inertia of cultural traditions and of social institutions must prevent any immediate all-over adjustment to changing conditions as would be required by a static model.

Therefore, in general we have to use dynamic models, which allow for slow, delayed or staggering adjustment, and which include the social conditions of earlier periods (and/or the time trends due to the changes going on in society) among their explanatory variables. This means that normally the explanation of a given cultural pattern or social institution will have to refer to the cultural patterns and social institutions of the previous period; and the conditions of the present will explain no more than the cultural and institutional *differences* between the present period and the previous one.

Very often when empirical observation

[1] As we are here concerned only with the basic logic of explanatory models, we shall neglect the distinction between differentials and finite differences, or between differential equations and difference equations.

[2] For, the equilibrium value of Y can be used to explain the actual value of Y only if Y is most of the time actually at or near its equilibrium value.

[3] See the previous footnote.

fails to discover any consistent relationships between two important groups of social variables, we can quite confidently expect to find meaningful relationships between *changes* in these two groups of variables. For instance, the examples of the United States, Great Britain and the Soviet Union show that there is no uniform connection between the productive technology of a society and its economic and political institutional framework, as in each of these societies basically the same productive technology is associated with very different economic and political institutions. In contrast, between *changes* in technology and the consequent changes in economic and political institutions (as well as the other way round) there do appear to exist well-defined recognizable relationships. For example, we already know a good deal about what economic and political changes tend to follow the adoption of Western technology by a non-Western country (and how the nature of these changes depends on the economic, political and cultural conditions of the country prior to Westernization). Similarly, we also have a good idea of the social and cultural changes that regularly tend to follow technological innovations in Western countries.

The point is that dynamic laws represent the general case of causal laws while static laws form a very special case, found only in causal systems of special descriptions[4]. Therefore society must be subject to dynamic laws if it is subject to causal laws[5] at all, but there is no reason to expect that it will be subject to static laws, except for certain special fields.

Greater care in distinguishing between static and dynamic models would no doubt enhance the theoretical value of certain analytical tools now commonly used by social scientists. For example, social institutions and cultural patterns are now often explained in terms of the *functional adjustment* hypothesis. It is argued that every society must display a certain minimum degree of functional efficiency, i.e. correspondence between social needs and social institutions—at least as much as is needed for the very survival of the society. This leads to the inference that all social institutions tend to serve some real social needs and that all important social needs tend to be catered for by some social institutions: the task of empirical research is simply to find out which social institutions are connected with which social needs and vice versa.

Another, closely related, analytical tool is the hypothesis of *cultural consistency* (see Benedict, 1934). It is assumed that the value-attitudes, beliefs and activity patterns of each society form a well-integrated self-consistent system. Once certain basic value-attitudes of the society are given, the other elements of the culture can be explained to a large extent in terms of this consistency requirement.

Both the functional adjustment and the cultural consistency hypotheses certainly contain a good deal of truth: there are strong forces that tend to adjust social institutions to social needs or alternatively to eliminate societies with extreme institutional maladjustments; and there are also strong forces that tend to iron out inconsistencies among the cultural patterns of a given society. But it is sufficiently clear that all these forces are often quite slow in operation (and often have to work in the face of strong counteracting forces which try to preserve maladjusted institutions or inconsistent cultural patterns) so that the requirements of a static model are not met. Consequently there is no presumption whatever to the effect that the institutions and cultural patterns of any given society will be found to be particularly well adjusted or particularly consistent. Undue insistence on such hypotheses can only lead to existing functional maladjustments or cultural inconsistencies being explained away. A satisfactory model of functional adjustment

[4] Any system subject to static laws is always also subject to dynamic laws but not vice versa. This is so because any static law can always be expressed in the form of a dynamic law, e.g. by adding a vacuous conditional clause involving a variable of the previous period, etc.

[5] This is true irrespective of whether these causal laws are deterministic or merely statistical.

and cultural integration cannot be static but rather has to include specific assumptions, at least in a rough qualitative sense, about the *speeds* at which the relevant adjustment and integration processes operate in different parts of the social system (as well as about the strength of the counteracting forces they have to overcome).

EXPLANATION AS A PROBLEM OF COMPARATIVE DYNAMICS

If society were a static mechanism the task of social science would be to explain differences and similarities between different societies or different parts of the same society in terms of certain basic social variables of the same period. That is, the fundamental problem would be a problem of *comparative statics*. As, however, society is a dynamic system the fundamental problem becomes a problem of *comparative dynamics*. The problem is to explain similarities and differences in the development over time of different societies or of different parts of the same society, in terms of the *initial conditions* (i.e. the conditions prevailing at some arbitrary point of time chosen as the starting point of our investigation) and in terms of the subsequent *external influences* (boundary conditions) affecting their development. On reflection it turns out that the explanation of *any* social fact is always a problem of comparative dynamics even if at first it is not stated in this form. For example, explaining the existence of industrial capitalism is the same thing as explaining why the Western society of the 18th century did produce a capitalistic industrial revolution while other societies, even those at comparable levels of economic and cultural development, did not have independent capitalistic industrial revolutions of their own. It was one of Max Weber's greatest contributions to social science to point out the reducibility of every question about explanation to a question in comparative dynamics.

Indeed, if an explanation proposed for a social fact does not explain why this fact appears or appeared in some societies or social groups but not in others—i.e., if it does not supply an explanation in a comparative dynamic sense—then we cannot accept it as full explanation of this social fact at all (though we may very well accept it as an important step towards a future explanation). For instance, even if the scapegoat theory of racial prejudice should be correct as far as it goes it cannot be regarded as full explanation of racial prejudice so long as it cannot explain why racial prejudice emerged in some societies or social groups but not in others, or why it is increasing in some social environments but is decreasing in others.

The fact that the social system is a dynamic system means that all problems in social science have an essential historical dimension and that in effect the main task of social science is to *explain historical development*. So it is most unfortunate that it has become customary for social scientists with a sense for theoretical analysis to neglect history and for social scientists with historical scholarship to keep themselves innocent of theoretical analysis.

Of course, in many cases the reluctance of historians to offer explanations (except on rather superficial levels) for the course of social development is simply a matter of there being no worthwhile analytical theory they could have drawn upon. For instance, so long as we have no theory to speak of on the dynamics of political institutions, we cannot blame historians if they fail to offer a satisfactory explanation for the fact that democracy works reasonably well in some countries but could never take firm roots in some other countries at comparable levels of cultural development. Even in economic history, progress in explaining the patterns of historical development was up to quite recently greatly hindered by the fact that available economic theory did not provide a satisfactory theory of economic growth. Only in the last few years have both economic theorists and economic historians shown a more active interest in explaining the differences in the rate and direction of economic development among various societies at different periods, and in setting up dynamic models consistent both with sound economic theory and with the established results of historical research.

As the main purpose of a comparative dynamic theory of society would be to explain the course of historical development, our present interest in comparative dynamics to some extent represents a return to problems that used to occupy the evolutionary theorists of the 19th and early 20th centuries—in contrast to the anti-theoretical, or at best static, approach fashionable in the more recent past (roughly in the inter-war period)[6]. But of course there are also fundamental differences. Evolutionists were looking for a law determining a uniform sequence of evolutionary stages for all societies. Comparative dynamics, on the contrary, is based on the idea that different societies often show very different patterns of development, and its main objective is to explain these differences in development in terms of differences in the initial conditions and the external influences.

Instead of assuming a law directly determining the course of social development (as a uniform sequence of evolutionary stages), a dynamic theory of society will have to explain social development in terms of the basic causal laws governing the interaction among the various social and environmental variables. It will have to set up analytical models based on specific—and, if possible, quantitative—assumptions concerning the causal influence that each major social and environmental variable has on the other variables, and concerning the causal mechanisms which transmit this influence. And of course ultimately all these causal laws will have to be explained in terms of interaction among the social groups—and, more fundamentally, among the individuals—who make up the social system.

EXOGENOUS VARIABLES AS EXPLANATORY VARIABLES

Among explanatory variables, special logical status belongs to variables *exogenous* to the social system, i.e., to the variables describing the natural environment of the society, and the biological properties of the population, in their aspects independent of human intervention. For, if a social scientist suggests an explanation for a social fact in terms of other social facts (i.e., in terms of variables endogenous to the social system), his explanation will be incomplete so long as he cannot offer explanation for these latter social facts themselves. But if he puts forward explanation for a social fact in terms of variables genuinely exogenous to the social system his analytical task will be completed, as it will not be his business as a social scientist to find an explanation for these exogenous variables themselves. In this sense the exogenous variables represent the only vehicles of "ultimate" explanation in social science.

For instance, if the differences between Western European and Indian economic development could be fully explained in terms of differences in the natural resources (as they in fact cannot), no further explanation would be required. But if we try to explain these economic differences, say, in terms of the religious differences (as between Christianity and Hinduism), this explanation will be incomplete so long as we cannot explain these religious differences themselves. Indeed, any explanation of the economic differences in terms of religion will carry full conviction only if there is some evidence that the religious differences themselves in turn can be explained by factors *independent* of the economic differences we have started from in the first place.

To be sure, experience shows that we very often cannot follow all causal chains back to the basic exogenous variables. In effect, if we want to restrict our analysis to a period shorter than the whole history of the human race, we have to admit as explanatory variables initial conditions which are already social variables; and if we want to limit our attention to a social system smaller than mankind as a whole, we have to admit as explanatory variables influences coming from other societies. But even apart from the natural limitation of the scope of our analysis in time and in space, the failure of all theories of environ-

[6] Economic theory in many ways followed a course different from that of most other social sciences in that it felt the influence of extreme evolutionist doctrines much less, and later largely avoided the anti-theoretical current that swept over sociology and anthropology. But it likewise went through a period of purely static theorizing and of neglect for development problems.

mental determinism (and the even more complete failure of theories of racial determinism) shows that our explanation of social development can hardly ever be so complete that we could avoid admitting "historical accidents," i.e., events that we can subsume only under statistical laws. This means that the connection between the known exogenous variables and the detailed course of social development has to be visualized in terms of stochastic (probabilistic) rather than deterministic models.

Of course, the concept of historical accidents must be handled with great care to make sure that its use will satisfy the requirements of probability theory. For instance, suppose that a given society did not make a certain invention in a given period even though there would have been a strong social need for it and even though all basic technological principles required were known. Then, as inventions are no doubt to some extent matters of luck, we may legitimately regard this failure to make the invention in question as a mere historical accident, if the period involved was reasonably short. But if the invention failed to occur over a long period the assumption that this was due to mere chance will have a very low probability, and a specific explanation, e.g., in terms of negative social attitudes towards invention, will be required.

But, even though explanation in terms of exogenous variables is not always possible, whenever it is possible it has a very special theoretical interest; and one of the basic tasks for a causal theory of the social system will be to discover the causal (deterministic or statistical) laws according to which the natural environment and the other exogenous variables influence the social variables, and to identify the causal mechanisms which transmit this influence. Not only the exogenous variables themselves have a privileged position as explanatory variables but so do all endogenous social variables which bear the direct impact of major exogenous variables and play a principal role in transmitting the latter's influence to the rest of the social system.

One of the reasons why explanation of social phenomena in terms of *economic* forces is often so fruitful lies in the fact that the economic system is one of the main channels through which the natural environment (in particular, the presence or absence of natural resources and of natural routes of communication) acts upon the social system. To be sure, the economic system is not the only causal channel through which the environment exerts its influence. For instance, geographical conditions have an important effect on the sizes and boundaries of political units; climate influences patterns of recreation and social intercourse, etc. In trying to explain differences in the development of different societies, of course, all these influences of the natural conditions have to be taken into account.

STABLE ENDOGENOUS VARIABLES (SOCIAL INSTITUTIONS AND CULTURAL TRADITIONS) AS EXPLANATORY VARIABLES

Social institutions and cultural traditions have an important role as explanatory variables fundamentally because of their *stability*.

Though only the exogenous variables are fully independent of the other variables, any stable endogenous variable also represents a relatively *independent source of causal influence* in that (so long as it maintains its stability) its value is independent of changes in the other variables.

The task of a dynamic theory of society is to explain social development in terms of the initial conditions associated with some arbitrary "initial" period (and in terms of the subsequent external influences). Hence special theoretical interest attaches to those causal channels which transmit the influence of the initial conditions to later periods. These causal channels are represented primarily by the stable variables of the social system. A given initial condition tends to have a persistent influence on later development either if it itself represents a relatively stable social variable (a stable social institution or cultural tradition) or if it has a lasting effect on another variable with a high degree of stability (e.g., the

social conditions, themselves possibly of short duration, which give rise to a lasting institution or cultural tradition). In effect, the influence of *any* initial condition can always be represented in principle by a constant parameter in the equations describing the behaviour of the system over time—though this constant parameter may not always correspond to a directly observable constant variable of the system.

To be sure, the effect of a stable social variable on social development, even if persistent, need not be large. In particular, variables connected with some causally isolated part of the social system may exhibit great stability—maybe precisely as a result of their isolation from influences coming from other parts of the system—and may still have very little influence on the rest of the system. For example, the social structure of an isolated rural community may remain practically constant for centuries without this fact having any important effect on society at large. But any social or cultural variable that tends *both* to show a high degree of stability *and* to remain in close interaction with other important variables can hardly fail to be a variable with a significant long-run influence. Even if the influence of such a stable variable should remain small in the short run, its long-run influence will tend to be considerable as it will represent the cumulative effect of a force persistently pressing in the same direction over a long period.

In view of the great causal importance of such relatively stable social institutions and cultural traditions, it is of particular interest to explain the behaviour of these institutions and traditions themselves—both their stability over time and the changes, if any, they display.

There are, of course, certain familiar stabilizing mechanisms which help to preserve all institutions and cultural traditions once they exist—such as education by teaching and example, social pressure for conformity, fear of the new and unfamiliar. But social scientists from Marx to the modern functionalists agree that an institution or cultural pattern cannot persist for long unless it satisfies some specific *interests* or psychological *needs* on the part of society as a whole or on the part of particular social groups. This means that the *stability* of all other social variables greatly depends on those *fundamental structural variables* which determine the common interests and psychological needs of the society as well as its division into the major social groups, and the separate interests and psychological needs of these groups themselves. These fundamental structural variables include the natural environment of the society and its way of utilization; the society's position among, and relation to, other societies; the prevailing basic value attitudes and the basic personality structure of the members of the society[7]; the social structure and its major subdivisions according to class, status, occupation, residential area, ethnic origin, religion, etc., as well as the basic economic, political and social relations among these groups. These fundamental structural variables, or rather changes in them, also play an important role in explaining *changes* in other social institutions and cultural patterns (if the relevant time lags are allowed for). Hence the differences between two societies in these fundamental structural variables must go a long way towards explaining the differences in their other social variables.

ACCUMULATIVE VARIABLES

If we apply the method of comparative analysis, not to two different societies, but rather to two different periods of the same society, obviously we cannot explain the observed differences in terms of completely stable social variables, which did not change from one period to the other. Rather, we have to rely on variables which did significantly change from one period to another but which at the same time remained approximately constant within each (short) period under comparison—that is, on variables which tend to change at a very slow rate but so that this adds up to large changes in the long run. Such variables may be called *accumulative* (or decumulative)

[7] The basic value attitudes and basic personality structure themselves may be determined by the prevailing type of family organization and child-rearing practices. See A. Kardiner, Linton, DuBois, and West (1945).

variables. Examples are: population, the stock of capital, the stock of natural resources (the latter is a "decumulative" variable subject to depletion), the "stock" of technological knowledge, the sizes of slowly growing social organizations, etc. Obviously accumulative variables of this sort represent very important causal links in explaining the fundamental long-run changes that occur in a society in the course of social development. In the short run they can be regarded as constant, and therefore as independent variables, while in the long run they are themselves subject to the moulding influence of other social variables.

These latter, i.e., the social and cultural conditions that decide the rate and direction of change in each of these accumulative variables, are of course another most important class of explanatory variables. For instance, it would be a major advance in economic theory if we could decide which are the main economic and social variables that determine the rate of increase in capital and in technological knowledge.

We have argued above that one reason for the fruitfulness of an economic analysis of social institutions lies in the fact that the economic system is one of the main causal channels through which the natural environment acts upon the social system. We can now add an additional reason: the economic system (if defined so as to include productive technology as one of its variables) contains at least three of the major accumulative variables, viz., capital, natural resources and productive technology. Therefore a good deal of the differences between the social conditions of two different periods can be explained in terms of economic differences. But of course a full explanation will have to bring in cumulative variables which are not "economic" in nature, e.g., population, the "stock" of pure scientific knowledge, military technology, communication and organization methods, etc.

INTERACTION BETWEEN DIFFERENT SUBSYSTEMS OF THE SOCIAL SYSTEM

Perhaps the most interesting methodological problem facing a social scientist is what working hypothesis to adopt about the likely direction of the causal influence between certain major groups of social variables (i.e., between different subsystems of the social system). Should we, with Max Weber, assign causal priority to the religious-ethical variables and explain the emergence of Capitalism in terms of Protestant ethics, or conversely should we, with Karl Marx, assign causal priority to the economic variables and explain Protestantism itself in terms of the economic conditions of late-medieval towns? Should we explain economic development in terms of political factors and regard Capitalism the by-product of the fiscal policies of national governments, or should we adopt the opposite explanation and make the national governments themselves the agents of Capitalistic economic interests?

Sometimes attempts have been made to dismiss all problems of this kind with the pronouncement that in society all causal relations are two-way affairs and that no social variable has causal priority over any other. But this is not, in my view, a fruitful approach. Maybe most causal relations run in both directions but no doubt one direction is often much more important than the other, and if so this fact will be very interesting information to us. In particular, so long as we have to work with very rough and often non-quantitative models, the only way of gaining insight into the causal structure of the social system is to find out the principal directions of causal influence among the major social and environmental variables.

In effect, hypotheses as to causal priority can always be translated into comparative dynamic hypotheses concerning the causal importance of the relevant social variables as initial conditions. For instance, Max Weber's theory is essentially a hypothesis on what difference it would have made to later economic development in Western Europe if Protestantism had not figured among the initial conditions of the relevant period. Likewise, the Marxian theory on the causes of the Reformation is a hypothesis about what difference it would have made to Western European religious development if the economic conditions of late-medieval towns had been different. Of course, without the possibility of experimental testing

(and we cannot set up a society similar to Western European society of the relevant period, except for the absence of Protestantism, and then observe what will happen), hypotheses of this sort can be supported only by indirect evidence. As in other nonexperimental sciences, the analysis of historical facts may be a partial substitute for experimentation. For example, Max Weber was able to test his thesis up to a point by comparing the economic development of the Western European society with that of other societies with different religions but otherwise more or less similar social institutions. But of course observation is never a full substitute for experimentation. The societies Max Weber used for comparison inevitably differed from Western European society in many other respects besides religion, e.g., in certain economic and political characteristics, and this involved the possibility of alternative interpretations for the historical facts.

However, this type of historical evidence may be supplemented by study of the causal mechanisms operative in social development and by investigation of the dynamic behaviour of the relevant social variables.

What does it mean to ascribe causal priority to one subsystem of the social system over another? It essentially means to assume that, while the main aspects of the first subsystem's development can be explained in terms of internal factors, i.e., in terms of interaction among its own variables, the second subsystem's development has to be explained in essential respects in terms of influences coming from the first subsystem. Obviously all parts of the social system and all parts of culture show some measure of relatively autonomous development and on the other hand none of them can claim full autonomy. But the various subsystems seem to exhibit conspicuous differences as to the degree of autonomy they possess. Philosophy, art, religion, law, etc., may for a while apparently follow their own internal logic in their development, but soon this development takes an unexpected turn, old ideas are abandoned for no apparent good reason and new ideas emerge which in no way represent a further development of the old. If an attempt is made to explain these developments in terms of their internal logic alone they remain profound mysteries. But if we bring in external factors, such as social, political and economic changes, things become at once meaningful and understandable. The economic system (and even more so the larger system including, besides the economic variables, also technology, political organization and the size and composition of the population) shows a much higher degree of autonomy. If someone wants to explain Capitalism as a product of Protestant ethics he has to find an explanation for the emergence of Protestant ethics itself—presumably in terms of the autonomous evolution of Christian theology. But Max Weber himself admits that Protestant ethics is by no means a logically necessary implication of Protestant theology; indeed he thinks that fatalistic ethics would have been logically more consistent with the doctrine of predestination. So the explanation of Protestant ethics in terms of autonomous theological developments fails at the very first step, and extra-theological social factors have to be invoked to explain why the early Puritans adopted the ethical attitudes so highly favourable to the development of Capitalism. On the other hand, if we try to explain Protestantism as a result of the social conditions prevailing in late-medieval towns, we have no fundamental difficulty in explaining how these social conditions themselves emerged as a result of economic, technological and political developments.

Over relatively short periods, of course, practically any subsystem of the social system may show largely autonomous developments, which may then have important effects on other parts of the social system. The point is only that in many subsystems such spells of relatively autonomous developments tend to be soon interrupted by outside influences. Indeed, as Kroeber (1944) has pointed out, many fields of culture simply do not possess the ability to display continual development in one particular direction indefinitely: if no outside influence intervenes they will soon

exhaust their internal developmental possibilities and simply stop or exhibit endless repetition of the same patterns.

CONCLUSION

To sum up, we have tried to show that the fundamental problem of social science is to explain social facts in terms of a *comparative dynamic theory* of social development.

We have argued that among explanatory variables special theoretical importance attaches to those which are relatively independent of the other variables of the social system, and which are therefore in a position to exert relatively *independent causal influence* on the other variables.

On the basis of this criterion we have discussed six groups of important explanatory variables, viz., (1) exogenous variables; (2) endogenous variables that mediate the influence of major exogenous variables; (3) endogenous variables with a high degree of stability (i.e. stable institutions and cultural traditions); (4) basic structural variables, which determine the values, interests and psychological needs of the members of the society and that way largely determine the stability of other social variables; (5) accumulative variables, connected with the basic changes of society in the long run; (6) the variables that determine the rate of growth of the accumulative variables.

We have used the same criterion of relative causal independence also for judging the likely direction of causal influence between different subsystems of the social system. We have argued that a subsystem is more likely to have causal priority over other subsystems the more it is intrinsically capable of persistent autonomous long-run movements of its own, and the more it is able to preserve these movements against disturbing influences coming from other parts of the social system.

REFERENCES

Benedict, Ruth. *Patterns of culture*. Boston: Houghton Mifflin Co., 1934.

Kardiner, A., Linton, R., DuBois, D., & West, J. *The psychological frontiers of society*. New York: Columbia Univ. Press, 1945.

Kroeber, A. L. *Configurations of culture growth*. Berkeley: Univ. of Calif. Press, 1944.

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The prosperous middle classes, who ruled the nineteenth century, placed an excessive value upon placidity of existence. They refused to face the necessities for social reform imposed by the new industrial system, and they are now refusing to face the necessities for intellectual reform imposed by the new knowledge. The middle-class pessimism over the future of the world comes from a confusion between civilisation and security. In the immediate future there will be less security than in the immediate past, less stability. It must be admitted that there is a degree of instability which is inconsistent with civilisation. But, on the whole, the great ages have been unstable ages.

A. N. WHITEHEAD, *Science and the Modern World*

The Sapir-Whorf hypothesis that the structure of a language influences cognitive behavior is questioned in this study of comparability of experience in different sense modalities. Do the Navajo, like ourselves, see HAPPY as more up and SAD as more down? Do the Japanese, like ourselves, conceive of EXCITEMENT as colorful and CALM as colorless? Do the Navajos and Anglos differ widely in their connotative meanings of the words "blue" and its correlate in Navajo, yet agree closely on their meanings for a specific BLUE color chip? Here is a first attempt to demonstrate that the visual-verbal synesthetic relationships characteristic of our own language/culture community are shared by peoples who speak different languages and enjoy different cultures. Perhaps there is a "world view" that is relatively stable despite differences in both language and culture.

THE CROSS-CULTURAL GENERALITY OF VISUAL-VERBAL SYNESTHETIC TENDENCIES

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INTRODUCTION

THIS study was planned as part of the Southwest Project in Comparative Psycholinguistics (Casagrande, 1956) during the summer of 1955. This project as a whole was concerned with the ways in which language or culture, or both, may produce differences in cognitive processes; *or*, conversely, the degree to which certain cognitive processes may be independent of differences in language or culture, and hence general across language/culture groups. One area of cognition studied was that of *connotative meaning*; specifically, translation-equivalent forms of the semantic differential (Osgood, Suci, & Tannenbaum, 1957) were given to subjects in several Southwest Indian communities, as well as to Mexican-Spanish and Anglo subjects. Suci (1957) has reported the results of this work elsewhere.

* The author designed this experiment, ran the Anglo control subjects, interpreted the results, and wrote them up. Dr. Susan Ervin collected data from the Navajo subjects; Dr. Sol Saporta administered the experiment to the Mexican-Spanish subjects; Mr. Hiroshi Azuma obtained data from Japanese subjects and analysed them. Mr. Murray Miron handled the analysis of the data (except for the Japanese) on IBM and ILLIAC. Without the help of these people, for which I am very grateful, this paper could not have been written.

The usual form of the semantic differential requires the subject to judge verbal concepts (e.g., HORSE, CORN, MAN) against verbally defined scales (e.g., *strong-weak*, *active-passive*, *good-bad*). Problems of translation equivalence therefore enter at two places. In the present experiment the concepts to be judged are verbal, but the 'scales' are visual—binary pictorial alternatives with which the subject must selectively associate the verbal concept being judged. Thus, instead of the bipolar words *thin-thick*, the subject sees a *thin* line paired with a *thick* line, and he simply points to whichever drawing seems to 'go best' with the concept being judged; instead of the bipolar adjectives *angular-rounded*, he sees a jagged, *angular* line-drawing paired with a *rounded* line-drawing. The difference between the drawings in each pair is restricted to a single dimension, e.g., angularity, size, nearness, etc. The verbal concepts judged against these pictorial alternatives included both concept-terms (e.g., MAN, YELLOW) and scale terms (e.g., STRONG, BAD, LIGHT, GOOD, WEAK) taken from the standard semantic differential used in the Southwest Project by Suci.

One purpose of the present study, then, was to check the generality of semantic

factors in a situation where at least the 'scales' of judgment were non-linguistic. Another purpose was to study the cross-cultural generality of visual synesthesia itself (and, indirectly, of metaphors based on visual analogies). Do the Navajo, like ourselves, see HAPPY as more *up* and SAD as more *down*? Do the Japanese, like ourselves, conceive of EXCITEMENT as *colorful* and CALM as *colorless*? And if certain differences in visual metaphor do appear, can these be related to what we know about the differences in culture? A third purpose was to see if those terms in Navajo, Mexican-Spanish, and Japanese selected as translation-equivalent to verbal opposites in English actually function as opposites in the meaningful judgments of non-Anglo subjects. Treating our paired visual alternatives as a sort of projective device, can the choices for GOOD be shown to be the mirror-image of those for BAD, for example? A final, and somewhat supplementary, purpose was to study similarities and differences in the connotations of both color terms and actual color samples. Can it be shown, for example, that Navajos and Anglos may differ widely in their connotative meanings of the words "blue" and its correlate in Navajo, yet agree closely on their meanings for a specific BLUE color chip? This analysis was restricted to the Navajo/Anglo comparison.

METHOD

Subjects

The synesthesia experiment was run on four groups of subjects, each representing a different language/culture base. Two of these groups, 40 Navajos and 10 Mexican-Spanish subjects, were included in the Southwest Project on Comparative Psycholinguistics (SWPCP), and the data were collected during the summer of 1956. Dr. Susan Ervin obtained the Navajo data and Dr. Sol Saporta obtained the Mexican-Spanish data. These subjects were, for the most part, rural people with little formal education. During the following year, the writer collected control data from a group of 27 Anglos, graduate and undergraduate students in psychology at the University of Illinois. The 20 Japanese subjects were either staff or students at the University of Illinois, but had been in this country less than two years; Mr. Hiroshi Azuma collected these data, as a research project in the author's course in psycholinguistics. It should be noted, first, that all but the Anglo subjects were bilingual to some degree with respect to English, and, second, that there are marked differences in education (and perhaps intelligence) between the Anglo and Japanese groups on the one hand and the Navajo and Mexican-Spanish on the other. The phenomena of visual synesthesia with which we are dealing here may be largely independent of these variables, but nevertheless they should be kept in mind in interpreting the results.

Materials

The verbal concepts used in this study are listed in Table 1. They include some of both the polar terms and the concepts used in the completely verbal semantic differential developed by Suci for SWPCP. They also include some color terms, for purposes of comparison with other experiments in the Southwest Project in which actual color chips were judged. Although the concepts are given here only in English, in administering the experiment the translation-equiv-

TABLE 1

VERBAL CONCEPTS USED IN SYNESTHESIA EXPERIMENT

1. HEAVY	*10. QUIET	*20. NOISY
2. GOOD	11. BLUE	*21. GREY
3. FAST	12. BAD	22. SLOW
4. HAPPY	13. LIGHT	23. WHITE
*5. UP	*14. DOWN	24. CALM
*6. ENERGETIC	15. BLACK	25. MAN
*7. LOOSE	16. WOMAN	26. YELLOW
8. STRONG	*17. LAZY	27. WEAK
9. EXCITEMENT	*18. TIGHT	28. SAD
	19. GREEN	

Pictorial Alternatives Used in Synesthesia Experiment
(see Figure 1)

1. up-down	5. thick-thin	10. rounded-angular
2. vertical-horizontal	6. dark-light	*11. diffuse-concentrated
3. homogeneous-heterogeneous	7. crooked-straight	12. large-small
4. colorless-colorful	8. hazy-clear	*13. near-far
	*9. blunt-sharp	

* These concepts and visual alternatives were omitted in the materials given to Mexican-Spanish subjects.

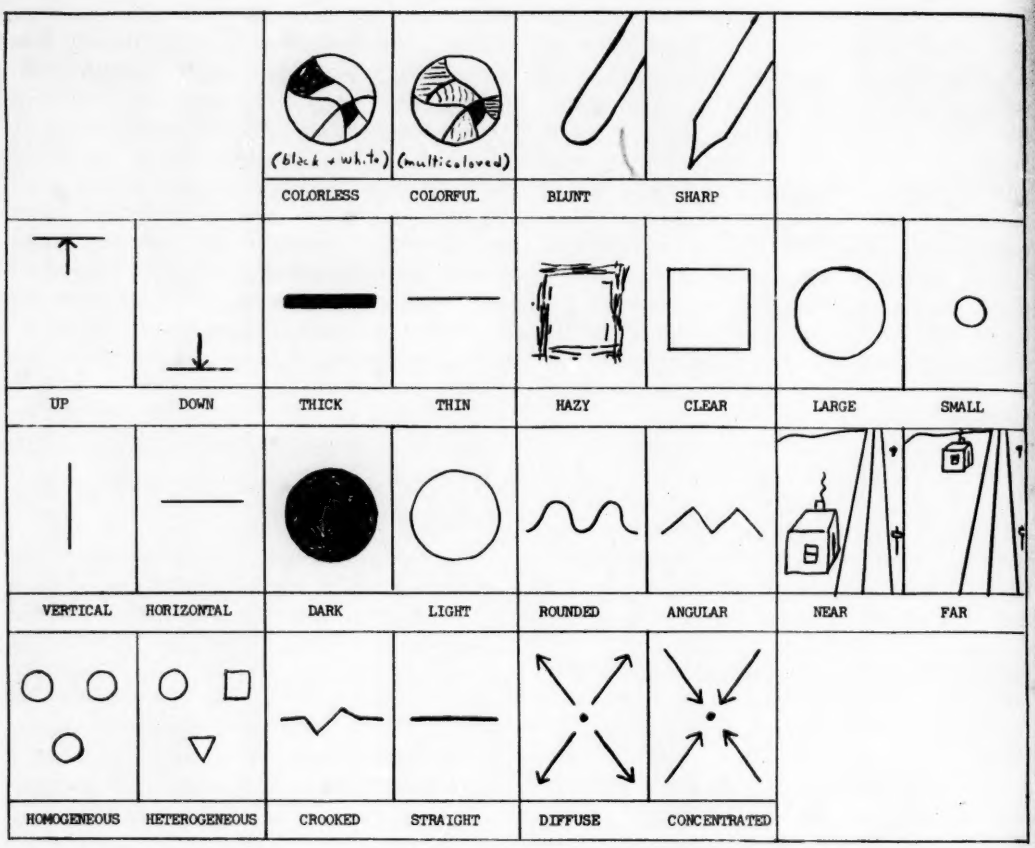

FIG. 1

alent terms in the subject's native language were given by the experimenter or interpreter. The equivalents for Japanese were determined by the back-translation technique (using different bilinguals in the two stages). Despite the extreme care exercised by the SWPCP linguists and by Mr. Azuma in achieving close translation-equivalence, perfect equivalence is probably impossible, and this is therefore another possible source of apparent differences in visual synesthetic tendencies across language groups.[1]

[1] Mr. Azuma, for example, reports several cases of only partial equivalence: the Japanese "karu-i," the equivalent of LIGHT, is used only with respect to weight, not also with respect to illumination, as in English; "reisei-na" (CALM) refers exclusively to emotional states, not being extended to weather, etc., as in English; "otoko" means male human and is not extended to human beings in general as is MAN in English; "haya-i," on the other hand, includes the notion of *early* as well as FAST.

The visual alternatives, or 'scales,' used in this study are displayed in Figure 1 and listed *verbally* in Table 1. The labelling of these visual alternatives is somewhat arbitrary and is done only to facilitate talking about them. The subjects were given no verbal characterizations, although they may well have done some spontaneous labelling. Each pair of visual alternatives was drawn on a single card, so that the order of presentation could be varied if desired; the left-right orientation on each card was as shown in Figure 1.

Procedure

The general procedure was to name one of the concepts to be judged and then run through the series of cards, having the subject point to or otherwise indicate which of the two visual alternatives on each card seemed most appropriate to that concept. Then the next concept would be named and

run through in the same fashion. Thus, in effect, the subject judged whether HEAVY (in his own language) seemed more *up* or *down*, *vertical* or *horizontal*, *homogeneous* or *heterogeneous*, and so forth through the 13 alternatives; then he did the same thing for the concept GOOD, and so on through the test.

There were some minor variations in procedure that may be noted, even though it is doubtful that they would have much effect on such a simple judgment process. For Navajo, Japanese, and Anglo subjects, the order of presenting the concepts was as given in Table 1; the order for the Mexican-Spanish subjects was different. Also, for the Mexican-Spanish group, certain concepts and 'scales' were omitted (those shown by asterisks in Table 1). The Navajo, Mexican-Spanish, and Japanese subjects were tested individually, each concept being spoken aloud before its series of judgments (and repeated whenever necessary). The Anglos were tested in two groups, the visual alternative cards being handed around from subject to subject in regular, repeating sequence; they indicated their judgments by marking "L" (left) or "R" (right) in the appropriate row, defined by printed concepts, and appropriate column, defined by number 1 to 13 for the 'scales.' For the Navajo and Japanese subjects the order of presenting 'scales' was randomized within subjects; for the Mexican-Spanish subjects the order of 'scales' was simply reversed on alternate concepts; for the Anglos the order was constant for all concepts. The direction, left to right, for each visual alternative card was constant for all subjects and groups.

RESULTS

Intra-cultural synesthetic tendencies

The first step in the treatment of the data was to compute the percentages of subjects in each group choosing each of the visual alternatives for each verbal concept. In other words, before comparing *across* language/culture groups, we want to determine the consistencies of synesthetic tendencies *within* each group. Table 2 presents these data. Since there were only two alternatives for each judgment, the proportion choosing one is always the reciprocal of the proportion choosing the other, and therefore only the percentage choosing the alternative listed at the top of each column is given, e.g., the fact that only .07 Anglos chose *up* means that .93 of them chose *down* as appropriate to HEAVY. To conserve space, the data for all four groups are included in this table, the first per cent in each quadruplet giving the Anglo data (A), the second the Navajo data (N), the third the Mexican-Spanish (S), and the last the Japanese data (J).

If we are to conclude that a particular synesthetic tendency is shared by the members of a cultural group, then we must show that a significant proportion of that group made the same choice. Accepting the 1% level of confidence as a conservative criterion of significant agreement within a group (except for the Mexican-Spanish, where the small N makes the 5% level seem more appropriate), then approximately 20/27 Anglo subjects (percentage values greater than .74 or less than .26 in Table 2), 28/40 Navajo subjects (percentages greater than .68 or less than .32), 8/10 Mexican-Spanish subjects (percentages greater than .79 or less than .21) and 16/20 Japanese subjects (percentages greater than .80 or less than .20) must choose the same way for the relationship to be considered non-chance.

Inspecting Table 2 with such criteria in mind, we find, for example, that Anglos agree among themselves that HEAVY is *down*, *colorless*, *thick*, *dark*, *concentrated*, and *near*; Navajos see HEAVY as *thick*, *dark*, *crooked*, *blunt*, *large* and *near*; Mexican-Spanish see it as *down*, *horizontal*, *heterogeneous*, *thick*, *dark*, *crooked*, *hazy*, and *large*; Japanese agree among themselves that HEAVY is *down*, *colorless*, *thick*, *dark*, *crooked*, *hazy*, *concentrated*, *large*, *near*, and *blunt*. The synesthetic associations for other concepts in Table 2 can be explored by the reader.

If these intra-cultural synesthetic tendencies were chance-like in their occurrence, we would expect about as many items to reach significance as the level of confidence we select, i.e., 1% of them. The fact of the matter is that 52% of all items for Anglos, 30% of all items for Navajos, 50% of all

TABLE 2

PERCENTAGES OF ANGLOS (A), NAVAJOS (N), MEXICAN-SPANISH (S) AND JAPANESE (J) CHOOSING THE VISUAL ALTERNATIVES DEFINING EACH COLUMN AS BEING APPROPRIATE TO THE VERBAL CONCEPT DEFINING EACH ROW.

	up	vert	homo	color-less	thick	dark	crooked	hazy	blunt	rounded	diffuse	large	near
HEAVY													
(A)	7*	37	37	78	93*	96*	30†	52	52	59	15	70	81*
(N)	33	65	48	50	95	85	73	63	75	45	35	87	80
(S)	10	20	20	40	90	100	100	90		70		100	
(J)	20	40	65	85	100	100	80	00	90	55	15	85	95
GOOD													
(A)	74	37	81*	11	15	26*	37	30	56	59	74	78	52
(N)	60	45	75	28	33	23	23	33	35	28	45	70	45
(S)	50	60	70	30	70	10	00	00		30		00	
(J)	45	25	90	35	25	5	00	10	45	40	50	70	95
FAST													
(A)	81	41	44	52	7*	22*	37	33	19	30	78*	22	22
(N)	50	38	50	60	30	23	40	25	43	40	70	58	58
(S)	60	60	90	80	00	20	00	40		30		10	
(J)	55	00	50	70	00	20	15	40	00	15	95	00	5
HAPPY													
(A)	89	70	63	7*	22	11*	41	41	56	78†	85	93	59
(N)	48	50	68	28	25	10	25	33	38	28	68	62	43
(S)	90	70	50	10	10	00	50	20		30		90	
(J)	60	25	95	10	45	5	00	25	95	15	45	70	80
UP													
(A)	96*	100	63	30	59	26	89	63	59	15	96*	52†	4
(N)	90	55	55	35	43	40	68	48	60	35	73	70	20
(S)													
(J)	100	95	65	20	15	10	60	60	50	40	100	15	00
ENERGETIC													
(A)	93	89	15†	22*	48†	37†	81	56	4	26	70	52	48
(N)	63	48	70	30	33	33	38	53	30	45	50	73	68
(S)													
(J)	85	75	65	10	95	85	70	25	55	55	85	100	80
LOOSE													
(A)	56	52	33	56	22	15	70	93*	81*	81*	93	70	70
(N)	35	45	38	58	45	35	75	68	83	75	28	43	55
(S)													
(J)	50	20	75	15	50	25	30	95	95	95	70	90	65
STRONG													
(A)	41	74	74	81	100	100	22	4	44	22	15	37	59
(N)	58	65	63	48	55	53	43	48	33	35	48	38	80
(S)	30	80	80	60	100	80	30	40		40		70	
(J)	25	55	65	90	90	90	65	5	65	15	35	70	85
EXCITE-MENT													
(A)	89	89	7	7*	52	52	96	81	15	22	67	67	56
(N)	44	53	51	26	51	36	53	51	36	47	44	75	56
(S)	80	80	20	10	70	70	90	90		40		100	
(J)	95	95	25	5	65	70	90	65	00	15	70	75	50
QUIET													
(A)	30	4*	93	81	22	41	00	37	100	96	41	37	26
(N)	30	33	58	55	40	35	35	53	45	40	58	23	50
(S)													
(J)	20	10	90	70	50	35	00	40	90	55	15	40	10
BLUE													
(A)	44	33	70	52	70†	93†	30†	59	78	70	44	63	26
(N)	53	50	41	15	47	29	73	70	44	44	44	50	56
(S)	90	60	60	60	70	60	60	50		70		30	
(J)	45	50	65	25	20	35	45	55	30	45	50	50	25

TABLE 2—*Continued*

	up	vert	homo	color-less	thick	dark	crooked	hazy	blunt	rounded	diffuse	large	near
BAD													
(A)	4	52	26*	89*	93*	96*	89*	67	41†	22†	15	26	48
(N)	43	58	28	83	80	83	85	88	73	73	43	33	38
(S)	10	40	10	90	100	100	100	100		50		60	
(J)	35	75	10	75	95	95	100	90	20	40	55	30	60
LIGHT													
(A)	100	67	52	15	00*	00*	41	70†	52	44	96	63	37
(N)	50	25	58	52	13	13	28	30	58	68	63	20	28
(S)	80	50	70	20	00	00	20	10		60		20	
(J)	80	55	55	25	5	5	25	95	15	70	85	25	5
DOWN													
(A)	00*	52	15	89	89†	93	89*	48†	33	44	19	33	63
(N)	13	45	45	65	38	40	70	63	48	63	35	38	85
(S)													
(J)	00	80	30	75	85	95	90	15	30	30	25	50	90
BLACK													
(A)	4	89	41	96*	100*	96*	52	37	19	48	00*	15	48
(N)	34	71	64	94	89	94	53	61	18	58	31	58	68
(S)	20	70	40	100	100	100	90	70		60		30	
(J)	25	70	40	100	100	100	80	5	45	45	5	35	80
WOMAN													
(A)	74	41	81	4*	26*	19*	56	67	89	93	48	44	48
(N)	50	50	58	35	30	8	50	58	58	55	35	35	38
(S)	80	30	60	10	50	40	70	40		70		50	
(J)	65	55	65	5	15	15	30	100	65	80	65	30	35
LAZY													
(A)	15	15	70	67	44	48	19†	59	100*	89	41	70	48
(N)	40	43	38	75	55	68	70	65	73	55	43	38	55
(S)													
(J)	25	35	65	40	60	65	60	95	95	90	70	80	45
TIGHT													
(A)	44	48	44	74	56	74†	33	7*	4	7*	7	7†	22
(N)	60	53	60	53	25	28	10	30	35	23	55	83	43
(S)													
(J)	30	80	25	85	65	85	85	5	00	00	15	5	45
GREEN													
(A)	48	48	59	4*	33†	33	44	56	59	56	78†	70	41
(N)	47	53	50	25	62	43	58	50	36	43	32	53	66
(S)	60	50	90	20	20	30	60	20		40		50	
(J)	40	40	80	5	15	5	30	55	60	90	75	50	50
NOISY													
(A)	74	89	15	19	70	56	93*	81	7	15	85	78	70
(N)	58	63	43	55	65	50	73	63	55	48	50	75	50
(S)													
(J)	70	70	15	15	70	65	90	70	5	30	75	65	85
GREY													
(A)	44	11	85	89	37†	37	48	93	81	74	52	67	41
(N)	37	27	63	48	27	31	69	69	73	63	63	37	51
(S)													
(J)	25	50	35	75	75	70	70	75	60	45	35	55	35
SLOW													
(A)	15*	26*	81	63	74†	67†	44	70	85*	93	44	78	44
(N)	35	35	53	53	28	13	38	45	73	53	43	45	58
(S)	10	20	30	50	80	80	70	80		80		70	
(J)	20	10	70	40	90	60	55	85	100	95	25	90	80
WHITE													
(A)	93	37	74	63	4*	4*	30	44	74	52	96	96	48
(N)	52	14	79	65	9	3	24	11	65	35	65	59	43
(S)	90	20	80	60	00	00	10	00		30		70	
(J)	65	40	75	90	00	5	15	45	45	60	85	85	35

TABLE 2—*Continued*

	up	vert	homo	color-less	thick	dark	crooked	hazy	blunt	rounded	diffuse	large	near
CALM													
(A)	41	7	100	59	26	15*	7	37	96	93	59	70†	30
(N)	29	29	64	42	39	31	39	51	72	51	53	34	47
(S)	50	10	80	60	30	20	10	30		60		70	
(J)	25	30	80	95	25	20	5	5	45	45	15	40	35
MAN													
(A)	63†	96	30	85	89*	89	48	22	26	22	52	67	67
(N)	64	71	56	58	68	56	61	44	41	44	50	71	71
(S)	60	90	20	90	70	80	30	60		20		90	
(J)	25	75	25	100	100	75	55	00	35	20	50	85	80
YELLOW													
(A)	74	56	37	15*	15	11*	59	78*	59	56	74	56	37
(N)	48	34	66	10	31	17	59	66	59	51	63	59	63
(S)	60	40	70	00	20	20	60	30		60		50	
(J)	65	40	60	5	30	20	45	80	50	65	80	50	50
WEAK													
(A)	52	19	48	56†	19*	26*	81	93	67	70	74	52	48
(N)	34	36	34	66	20	36	71	56	64	66	49	31	36
(S)	30	70	60	30	10	10	80	50		60		30	
(J)	60	70	45	00	10	10	50	100	65	75	80	30	20
SAD													
(A)	7	11	59	93*	70	85	41	85	78†	74	11	33	33
(N)	47	36	33	69	69	47	60	63	63	58	50	31	47
(S)	00	30	00	100	90	100	70	100		60		10	
(J)	25	70	5	100	55	65	65	90	25	40	55	5	25

* Items where all three groups (Mexican-Spanish excluded) make the same choices at the 1% level of significance.

† Items where one of the three groups (Mexican-Spanish excluded) makes a significant (1% level) deviant choice.

items for Mexican-Spanish, and 44% of all items for Japanese reach this conservative criterion. In other words, when 28 verbal concepts are judged against 13 different visual alternatives in all possible combinations (364 items), approximately half of the items yield evidence for consistent intracultural synesthesia.

Cross-cultural synesthetic tendencies

The principal purpose of this research was to explore similarities and differences in synesthetic tendencies across language/culture groups. To the extent that peoples in two cultures share a common basis for synesthesia, the deviations from a 50/50 selection of alternatives should be in the same direction. Thus if more Anglos judge EXCITEMENT to be *large* rather than *small,* more Navajos should make this choice rather than the reverse, and similarly throughout all items and all group comparisons. Our first test of shared synesthetic tendencies, then, is to compute, for each pair of groups, the percentage of items yielding the same direction of choice. For Ango *vs.* Navajo groups we find 65% of 364 items agreeing in direction; for Anglos *vs.* Mexican-Spanish 72% of 190 items agree; for Anglos *vs.* Japanese 78% of 364 items agree; for Navajo *vs.* Mexican-Spanish 61% of 190 items agree; for Navajo *vs.* Japanese 71% of 364 items agree; and for Mexican-Spanish *vs.* Japanese 69% of 190 items agree in direction. Although these percentages may not appear impressive in size, they are nevertheless significant at beyond the .005 level of confidence. The most conservative test of significance was used here, counting items where choices were 50/50 with the disagree proportion and using a two-tail test.

The preceding test includes many items where the synesthetic tendency in one or

both groups is essentially chance, and gives all items equal weight (e.g., a 19/21 split counts as much as a 2/38 split). A more meaningful test, therefore, is to count only those items for which *both* groups being compared yield significant within-group agreement. In other words, if the subjects in each of two different language/culture groups agree among themselves as to the direction of synesthetic association, is the direction favored the same for both groups?

For Anglos *vs.* Navajos there were 73 items where both groups yielded significant within-group agreement; of these 86% were in the same direction of synesthetic association. For Anglo *vs.* Mexican-Spanish, 67 items met these criteria and 100% of them were in the same direction of association. For Anglos *vs.* Japanese, 98 items were significant for both and 99% (all but one) were in the same direction. For Navajos *vs.* Mexican-Spanish, we have 49 common significant items yielding 96% agreement. For Navajos *vs.* Japanese 90% of 71 shared

TABLE 3

SIGNIFICANT (APPROX. .01 LEVEL) SYNESTHETIC AGREEMENTS AND DISAGREEMENTS AMONG ANGLO, NAVAJO AND JAPANESE GROUPS

ANGLOS, NAVAJOS AND JAPANESE AGREE THAT:		ANGLOS, NAVAJOS AND JAPANESE DISAGREE ON WHETHER:	
HEAVY	is down, thick, dark and near.	HEAVY	is crooked (A) or straight (N, J)
GOOD	is homogeneous and bright.		
FAST	is thin, bright, and diffuse.		
HAPPY	is colorful and bright.	HAPPY	is rounded (A) or angular (N, J).
UP	is up and diffuse.	UP	is large (N) or small (J).
ENER-GETIC	is colorful.	ENER-GETIC	is heterogeneous (A) or homogeneous (N); is thin and light (N) or thick and dark (J).
LOOSE	is hazy, rounded and blunt.		
STRONG			
EXCITE-MENT	is colorful.		
QUIET	is horizontal.		
BLUE		BLUE	is thick, dark and straight (A) or thin (J), or light and crooked (N).
BAD	is heterogeneous, colorless, thick, dark and crooked.	BAD	is angular (A) or rounded (N); is blunt (N) or sharp (J).
LIGHT (weight)	is thin and bright.	LIGHT	is clear (N) or hazy (J).
DOWN	is down and crooked.	DOWN	is thick (A, J) or thin (N); is hazy (N) or clear (J).
BLACK	is colorless, dark, thick, and concentrated.		
WOMAN	is colorful, thin and bright.		
LAZY	is blunt.	LAZY	is straight (A) or crooked (N).
TIGHT	is clear and angular	TIGHT	is dark and small (A, J) or bright and large (N).
GREEN	is colorful.	GREEN	is thin and diffuse (A, J) or thick and concentrated (N).
NOISY	is crooked.		
GREY		GREY	is thin (N) or thick (J).
SLOW	is down, horizontal and blunt.	SLOW	is thick (A, J) and dark (A) or thin and bright (N).
WHITE	is thin and bright.		
CALM	is bright.	CALM	is large (A) or small (N).
MAN	is thick.		
YELLOW	is colorless, bright and hazy.		
WEAK	is thin and bright.	WEAK	is colorless (N) or colorful (J).
SAD	is colorless.	SAD	is blunt (A, N) or sharp (J).

significant items show the same direction of choice. For the Mexican-Spanish/Japanese comparison, 97% of 58 common significant items agreed in direction. It is clear that when we count only those items for which the groups being compared both show *intra*-cultural consistency, the *cross*-cultural agreement becomes very impressive indeed. Although the Navajo group shows slightly lower agreement with the other groups, all of these percentages are significant at well beyond the .001 level of confidence.

Having demonstrated over-all generality of synesthetic relationships, we may now consider specific agreements and—particularly—disagreements. Table 3 summarizes this information. It is restricted to comparisons among Anglos, Navajos, and Japanese; not only does the small N for Mexican-Spanish limit the possible significance of comparisons, but the other three groups represent extreme variations in both language families and culture. While inspecting Table 3 it should be kept in mind that *thick*, *near*, *homogeneous*, etc., refer to the visual alternatives shown in Figure 1.

A few of the significant agreements are artifactual, e.g., that UP is *up* and that BLACK is *colorless*. But most of the significant agreements across the Anglo, Navajo, and Japanese groups bring out what we feel intuitively to be appropriate synesthetic relations in our own languages of aesthetics and metaphor. Thus FAST is *thin*, *bright*, and *diffuse*; LOOSE is *hazy*, *rounded* and *blunt*; ENERGETIC and EXCITEMENT are *colorful*; BAD is *heterogeneous*, *colorless*, *thick*, *dark*, and *crooked*; NOISY is *crooked*; SLOW is *down*, *horizontal*, and *blunt*; and so forth.[2] The visual alternatives selected here are not denoted by the verbal concepts and their translation equivalents; rather, they are connoted on a synesthetic (or metaphoric) basis.

The significant disagreements are open to a number of different interpretations: (1) Differences in the metaphorical extensions of the visual dimensions themselves. This may be the case as between Anglos and Japanese, on the one hand, and Navajo on the other, for the scales *thick-thin*, *dark-light* and *straight-crooked*. (2) Differences in the denotative meanings of verbal concepts, and hence unsuccessful translation. This is certainly the case for Anglo *vs.* Navajo meanings of BLUE (cf., results for color study below). (3) Differences in the connotative implications of translation-equivalent verbal concepts. This may be the case for Anglo *vs.* Navajo meanings of ENERGETIC, LAZY, FAST, SLOW, and TIGHT. It is obviously impossible to choose among these alternative interpretations on the basis of these data alone. In passing it may be noted that there were almost four times as many significant disagreements between Navajo and the other two groups as between Anglo and Japanese.

[2] The one case of cross-cultural agreement that goes against the writer's intuitive grain is that of CALM being seen as *bright*, but in this he must be idiosyncratic.

Correlation matrices

The percentage table for each group, combined as Table 2, can be used to generate two correlation matrices: (A) *Concept correlation matrix*. Here we correlate each concept with every other concept (rows of Table 2) across the n visual dimensions (columns), n being 13 for Anglo, Navajo, and Japanese correlations and 10 for the Mexican-Spanish correlations. This generates a 28/28 matrix of intercorrelations for the three main groups and a 19/19 matrix for the Mexican-Spanish. Here we are using the visual alternatives as a set of 'ink blots' and we want to know how the various verbal concepts relate to each other. Do GOOD and ENERGETIC, for example, 'mean' more nearly the same thing to the Navajo than to American Whites, in the sense that the pattern of choices by the Navajo for these concepts is more similar? (B) *Visual-alternative correlation matrix*. Here we correlate each visual dimension with every other (columns) across the set of N concepts (rows), N being 28 for the three main groups and 19 for Mexican-Spanish. This generates a 13/13 (or 10/10 for Mexican-Spanish) matrix of intercorrelations. Here we are interested in relationships among the visual dimensions themselves. Does the selection of *dark* vs. *light* tend to correlate with the selection of *thick* vs. *thin*, for example, across the set of verbal concepts for all four groups? Due to the magnitude of these

TABLE 4

CORRELATIONS BETWEEN STANDARD VERBAL OPPOSITES (IN ENGLISH) ACROSS VISUAL ALTERNATIVES FOR ANGLO, NAVAJO, MEXICAN-SPANISH AND JAPANESE SUBJECTS

	ANGLO	NAVAJO	MEXICAN-SPANISH	JAPANESE
HEAVY-LIGHT	−.87	−.82	−.81	−.96
GOOD-BAD	−.92	−.91	−.55	−.75
FAST-SLOW	−.61	.47	−.73	−.52
HAPPY-SAD	−.73	−.66	−.89	−.85
UP-DOWN	−.39	−.72		−.58
ENERGETIC-LAZY	−.91	−.82		−.16
LOOSE-TIGHT	−.84	−.58		−.96
STRONG-WEAK	−.89	−.71	−.45	−.90
EXCITEMENT-CALM	−.75	−.21	−.54	−.72
BLACK-WHITE	−.82	−.47	−.75	−.55
MAN-WOMAN	−.86	−.35	−.70	−.91
QUIET-NOISY	−.95	−.62		−.92

correlation tables, they are not reproduced here,[3] but the results are summarized in the factor analyses to be reported in a later section.

(A) Concept matrices. One specific question that can be answered from the concept correlation matrices is this: do terms that are translation-equivalent to standard verbal opposites in English function as semantic opposites in Navajo, Mexican-Spanish, and Japanese? If they do function in this fashion, then the profile of choices for GOOD, say, should be reciprocal to that for BAD and hence yield a high negative correlation across the visual alternatives. Table 4 summarizes the data on this question, i.e., the correlations between concepts that are standard opposites in English for the various language/culture groups. The consistent negative *r*'s in this table permit us to conclude with confidence that Navajo, Mexican-Spanish, and Japanese groups do treat these concepts functionally as opposites. If one visual alternative is chosen as appropriate to HAPPY (say, the *light* circle), we can predict that the other alternative will be chosen as appropriate to its opposite, SAD (the *dark* circle). Since the alternatives associated with the verbal concepts are not themselves verbal, and further, since the verbal opposites appeared independently and widely separated in the test, this seems to be an adequate demonstration of functional opposition. The oppositions are not as polar for the Navajo as for the other groups, but whether this is due to lower reliability, to inaccurate translation, or to a less clear conception of opposition cannot be told from these data. Several exceptions to the rule may be noted in Table 4: The glaring exception is the *positive* correlation between FAST and SLOW for the Navajo; we will consider this in discussion. It can also be seen that UP-DOWN functions less clearly as an opposition for Anglos than for the other groups, that ENERGETIC-LAZY is hardly a functional opposition for the Japanese, and that EXCITEMENT-CALM and MAN-WOMAN are less clear oppositions for the Navajo than for the other groups.

Highly significant agreements across all language/culture groups for other than direct opposite relations are shown in Table 5 (A). Again, because of the small sample and missing items, the Mexican-Spanish data are not included (but inspection indicates that with no exceptions the relations shown here also apply to this group). To facilitate visual inspection, relations are repeated in clusters for each verbal concept (e.g., the GOOD/HAPPY correlations appear both in the GOOD cluster and in the HAPPY cluster). Some of these consistent relations seem to reflect nothing more than we would expect from the commonness of human beings and their physical environments: it is GOOD to be HAPPY and BAD to be SAD; LIGHT (weight) things tend to be FAST and EXCITEMENT tends to be NOISY; TIGHT musculature is associated with STRONG and LOOSE musculature with WEAK; MAN tends to be STRONGer than WOMAN, and WOMAN is LIGHTer (weight). Other consistent relations across these cultures suggest 'true' shared metaphors: subjects tend to make the same

[3] These data have been deposited as Document number 6240 with the ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington, 25, D.C. For a copy, cite document number and remit $2.50 for photoprints, or $1.75 for 35 mm. microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

synesthetic choices for WHITE as they do for GOOD, CALM, and FAST; opposed synesthetic choices are made for LIGHT (weight) vs. BLACK (i.e., BLACK has a "heavy" connotation); the choices for WOMAN are similar to those for YELLOW, but opposed to those for BLACK.

Table 5 (B) lists the most extreme cases of disagreement among these three language/culture groups in concept correlations. Since the disagreements clearly cluster in terms of certain key verbal concepts, the table is organized on the basis of such clusters. The Navajo use ENERGETIC in the same visual metaphors as they do GOOD, STRONG, HAPPY, and TIGHT (and the

TABLE 5

A. CONCEPT CORRELATIONS FOR WHICH ANGLOS, NAVAJOS AND JAPANESE DISPLAY *Agreement* IN DIRECTION ($r > \pm.50$)

	A	N	J		A	N	J
GOOD/HAPPY	.88	.86	.81	BAD/HAPPY	−.93	−.87	−.87
GOOD/SAD	−.56	−.77	−.74	BAD/SAD	.57	.80	.80
GOOD/WHITE	.78	.59	.52	BAD/WHITE	−.82	−.63	−.64
HAPPY/GOOD	.88	.86	.81	SAD/GOOD	−.56	−.77	−.74
HAPPY/BAD	−.93	−.87	−.87	SAD/BAD	.57	.80	.80
				SAD/ENERGETIC	−.73	−.81	−.60
LIGHT (wt.)/FAST	.70	.50	.57				
LIGHT (wt.)/BLACK	−.82	−.55	−.81				
LIGHT (wt.)/WOMAN	.53	.53	.81				
FAST/LIGHT (wt.)	.70	.50	.57				
FAST/WHITE	.60	.82	.60				
ENERGETIC/ EXCITEMENT	.94	.65	.56				
ENERGETIC/SAD	−.73	−.81	−.60				
EXCITEMENT/ ENERGETIC	.94	.65	.56	CALM/QUIET	.90	.68	.61
EXCITEMENT/NOISY	.95	.52	.88	CALM/WHITE	.62	.52	.55
TIGHT/STRONG	.83	.66	.62	LOOSE/STRONG	−.91	−.53	−.51
				LOOSE/WEAK	.83	.75	.51
STRONG/TIGHT	.83	.66	.62	WEAK/LOOSE	.83	.75	.51
STRONG/LOOSE	−.91	−.53	−.51				
STRONG/MAN	.71	.80	.83	WEAK/MAN	−.79	−.65	−.82
MAN/STRONG	.71	.80	.83	WOMAN/LIGHT (wt.)	.53	.53	.81
MAN/WEAK	−.79	−.65	−.82	WOMAN/YELLOW	.65	.61	.87
				WOMAN/BLACK	−.66	−.57	−.84
NOISY/EXCITEMENT	.95	.52	.88	QUIET/CALM	.90	.68	.61
WHITE/GOOD	.78	.59	.52	BLACK/LIGHT (wt.)	−.82	−.55	−.81
WHITE/FAST	.60	.82	.60	BLACK/WOMAN	−.66	−.57	−.84
WHITE/CALM	.62	.52	.55	BLACK/YELLOW	−.80	−.70	−.81
WHITE/BAD	−.82	−.63	−.64				
YELLOW/BLACK	−.80	−.70	−.81				
YELLOW/WOMAN	.65	.61	.87				

TABLE 5—*Continued*

B. SOME EXTREME *Disagreements* IN CONCEPT CORRELATIONS BETWEEN ANGLOS, NAVAJOS AND JAPANESE

	A	N	J		A	N	J
ENERGETIC/GOOD	.05	.87	.30	LAZY/GOOD	.14	−.84	−.10
ENERGETIC/STRONG	−.26	.76	.28	LAZY/BAD	−.14	.84	−.13
ENERGETIC/TIGHT	.02	.69	.06	LAZY/HAPPY	−.04	−.80	.27
				LAZY/SAD	.65	.77	−.10
ENERGETIC/NOISY	.89	−.08	.48	LAZY/NOISY	−.80	.04	−.27
TIGHT/HAPPY	−.66	.78	−.73	LOOSE/HAPPY	.49	−.44	.72
TIGHT/BAD	.58	−.78	.69	LOOSE/BAD	−.44	.55	−.59
TIGHT/DOWN	.52	−.44	.81	LOOSE/DOWN	−.39	.64	−.66
TIGHT/GOOD	−.56	.83	−.44	LOOSE/SAD	−.19	.57	−.53
TIGHT/BLACK	.76	−.14	.82				
TIGHT/WHITE	−.49	.62	−.48				
TIGHT/SAD	.21	−.67	.53				
MAN/BAD	.44	−.39	.37				
MAN/DOWN	.49	−.10	.79	WOMAN/DOWN	−.68	.16	−.80
MAN/CALM	−.64	−.69	.20				
FAST/ENERGETIC	.46	.54	−.46	SLOW/CALM	.64	.67	.06
FAST/HAPPY	.39	.68	−.16	SLOW/LIGHT (wt.)	−.45	.62	−.35
				SLOW/WHITE	−.10	.67	−.21
STRONG/GOOD	−.44	.66	.12				
STRONG/BAD	.48	−.68	.21				
STRONG/HAPPY	−.56	.47	−.14	EXCITEMENT/ SAD	−.63	−.56	.08
STRONG/SAD	.25	−.71	−.03				
STRONG/EXCITE-MENT	−.34	.69	−.13				
BLUE/EXCITEMENT	−.55	.60	.26	GREEN/HAPPY	.80	−.02	.70
BLUE/QUIET	.58	−.22	−.04	GREEN/LIGHT (wt.)	.70	−.64	.57
BLUE/NOISY	−.56	.50	.02	GREEN/WHITE	.53	−.45	.46
BLUE/SAD	.67	−.05	−.22	GREEN/BLACK	−.78	.18	−.74
BLUE/ENERGETIC	−.67	.18	−.02	GREEN/DOWN	−.73	.20	−.66
BLUE/LAZY	.62	−.14	.15	GREEN/HEAVY	−.53	.55	−.45
BLUE/YELLOW	−.38	.70	.64	GREEN/STRONG	−.63	.52	−.64
BLUE/GREEN	.01	.66	.59	GREEN/MAN	−.55	.60	−.68
GREY/CALM	.73	.80	−.08	YELLOW/STRONG	−.89	.11	−.83
GREY/MAN	−.69	−.74	.32	YELLOW/TIGHT	−.75	.10	−.77
GREY/WOMAN	.34	.61	−.40				
GREY/WEAK	.65	.74	−.28				

reverse for LAZY); for Anglos and Japanese these concepts are essentially unrelated, except that ENERGETIC goes with NOISY (and LAZY the reverse), which is not the case for Navajos. Similarly we find the Navajo using TIGHT and LOOSE in clearly different metaphorical relations than do Anglos and Japanese, TIGHT being used like HAPPY, GOOD, and WHITE (and the reverse for LOOSE); the other two cultures reverse these relations systematically. MAN behaves like DOWN, and WOMAN more like UP, for Anglos and Japanese, but there is no relation for the Navajo; yet Anglos and Navajos agree, in contradistinction to the Japanese, in seeing MAN as unlike CALM. The FAST/SLOW relations are confused—the Japanese are

TABLE 6

A. VISUAL-ALTERNATIVE CORRELATIONS FOR WHICH ANGLOS, NAVAJOS, JAPANESE AND MEXICAN-SPANISH DISPLAY *Agreement* IN DIRECTION ($r > \pm.35$)

	A	N	J	MS
up/colorful	.75	.36	.68	.51
up/diffuse	.83	.44	.75	—
vertical/sharp	.73	.44	.51	—
homogen./straight	.74	.73	.78	.66
colorless/dark	.64	.54	.51	.57
thick/dark	.94	.90	.92	.91
thick/concent.	.73	.54	.59	—
dark/concent.	.84	.47	.55	—
blunt/rounded	.87	.54	.73	.00*
large/near	.36	.37	.65	—

* M-S data not used as criterion.

B. SOME EXTREME *Disagreements* IN VISUAL-ALTERNATIVE CORRELATIONS BETWEEN ANGLOS, NAVAJOS, JAPANESE AND MEXICAN-SPANISH GROUPS

	A	N	J	MS
N and MS vs. A and J				
homogeneous/angular	−.65	.58	−.54	.22
colorless/hazy	−.21	.16	−.52	.44
thick/hazy	−.20	.58	−.31	.70
dark/hazy	−.27	.56	−.32	.88
dark/rounded	−.29	.26	−.45	.30
crooked/rounded	−.48	.61	−.38	.53
angular/large	−.33	.67	−.31	.31
N vs. A, J, and MS				
vertical/large	−.04	.59	−.18	.05
heterogeneous/blunt	−.77	.41	−.66	—
crooked/blunt	−.49	.43	−.33	—
clear/diffuse	−.33	.49	−.56	—
sharp/large	−.35	.45	−.49	—
A vs. N, J, and MS				
dark/crooked	.04	.56	.82	.63
diffuse/large	.63	.03	−.09	—
diffuse/far	.08	.48	.43	—
J vs. A, N, and MS				
hazy/far	−.08	−.16	.46	—
hazy/crooked	.52	.85	.04	.76
N vs. J (others neutral)				
colorful/rounded	−.04	−.43	.49	.09

the divergent group in FAST/ENERGETIC, FAST/HAPPY, and SLOW/CALM correlations, but the Navajo diverge from the other two groups for the SLOW/LIGHT (weight) and SLOW/WHITE correlations. The Navajo see STRONG as most like the positive evaluators, GOOD and HAPPY, and like EXCITEMENT; the Anglos, surprisingly, see STRONG as more like negative evaluators; the Japanese treat STRONG as essentially independent of (uncorrelated with) evaluative concepts. On the color terms we find very systematic differences: the concept BLUE is clearly used differently by the Anglos than by the Navajos and Japanese; by the former it is used like QUIET, SAD, and LAZY and is uncorrelated with YELLOW or GREEN; by the latter it is used like the other color terms. The concepts GREEN and YELLOW, on the other hand, are handled similarly by the Anglos and Japanese, but 'deviantly' by the Navajo, who see GREEN as related to HEAVY, STRONG, and MAN and who see YELLOW as independent of STRONG and TIGHT (this color being significantly NOT-STRONG and NOT-TIGHT for the other two groups). Finally, the Japanese are 'deviant' in the use of GREY, failing to use it like CALM, WEAK, and WOMAN, as do the Anglos and Navajo.

(B) Visual-alternative matrices. These correlation matrices indicate which visual dimensions tend to be used the same or different ways in differentiating the 28 verbal concepts. Table 6 (A) lists the *agreements* across groups in use of visual dimensions and Table 6 (B) gives the most notable *disagreements*. Since the data for the Mexican-Spanish seem particularly relevant here, and were based on correlations over 19 concepts, they are also included. The clearest relationships within the visual frame of reference for all groups are that *up* goes with *colorful* and *diffuse*; *dark* goes with *colorless*, *thick*, and *concentrated*; and *thick* goes with *dark* and *concentrated* (and the opposites of these terms, of course, showing the reverse relations). Other specific agreements: *vertical* functions like *sharp*; *homogeneous* is seen as being like *straight* (not *crooked*); while *blunt* is seen to be like *rounded* (not *angular*); and, quite reasonably, *large* goes with *near*. These relationships within the visual framework will be found (below) to contribute heavily to the major factors extracted from the visual-alternative matrices.

The disagreements among language/

culture groups shown in Table 6 (B) are interesting. The largest number of disagreements separate the Navajo and Mexican-Spanish, on the one hand, from the Anglos and Japanese on the other—which could be due to either geographical (visual locale) or education-literacy differences. Navajos and Mexicans in the Southwest agree (in contradistinction to Anglos and Japanese): in seeing *homogeneous* as like *angular*; *colorless*, *thick*, and *dark* as being like *hazy*; *dark* and *crooked* as being like *rounded*; and *angular* as being *large*. Navajos 'deviate' from the other groups: in relating *vertical* with *large*; *heterogeneous* (rather than *homogeneous*) and *crooked* (rather than *straight*) with *blunt*; *clear* with *diffuse*; and *sharp* with *large*. It is possible that the Mexican-Spanish would have agreed with the Navajo on most of these relations also, but they were not tested on most of these terms. Anglos 'deviate' from the other groups on three items: they do not see *dark* as being liked *crooked*, or *diffuse* as being like *far*, but rather they see *diffuse* as like *large*, which the other groups do not. Japanese 'deviations' are restricted to the *hazy* dimension, this visual polarity being associated with *far* but not with *crooked* as in other groups (non-evaluative for Japanese?). Japanese and Navajo differ in seeing *colorful* as *rounded* (J) or *angular* (N). In interpreting these similarities and differences in the visual frame of reference, the reader should refer back to the diagrams in Figure 1 and keep in mind that it was these drawings, not words, to which the subjects were reacting.

Correlations between matrices

One way of getting an over-all picture of the similarities between groups in synesthetic tendencies is to correlate their intercorrelation matrices cell by cell. To the extent that the matched coefficients covary throughout the two matrices being compared, we have evidence that common variables are operating. This estimate of similarity has the advantage of being independent of the absolute magnitudes of the *r*'s in the two matrices; it has the disadvantage that lack of independence between the rows and columns makes it impossible to estimate significance levels of the *r*'s. However, gross differences in the similarities of groups can be indicated in this manner. Table 7 gives such over-all correlations for both concept matrices and visual-alternative matrices. The first thing to note is that all of these correlations are positive and, for the most part, of reasonable size. This supports the conclusion reached earlier on the basis of item analyses that there are considerable similarities in synesthetic tendencies across language/culture groups. The second thing to note is that whereas Navajo is the one deviant group for correlations of *concept* matrices (markedly lower *r*'s with all other groups), both the Navajo and Mexican-Spanish groups separate sharply from the Anglo and Japanese groups in the correlations of *visual-alternative* matrices. Another way of expressing this finding is to say that the Mexican-Spanish shift their 'allegiance' from the Anglo on concept relations to the Navajo on scale relations. This is a rather remarkable contrast.

TABLE 7

CORRELATIONS BETWEEN INTERCORRELATION MATRICES FOR ANGLO, NAVAJO, MEXICAN-SPANISH AND JAPANESE GROUPS

	CONCEPT MATRICES		
	Navajo	Mexican-Spanish	Japanese
Anglo	.43	.75	.67
Navajo		.41	.24
Mexican-Spanish			.68
	VISUAL-ALTERNATIVE MATRICES		
	Navajo	Mexican-Spanish	Japanese
Anglo	.37	.43	.76
Navajo		.81	.39
Mexican-Spanish			.42

Factor analyses

Our final interest in these data lay in the structure or system among both verbal concepts and visual dimensions, as displayed in this synesthesia task. To what extent can

TABLE 8
ROTATED FACTOR LOADINGS FOR CONCEPT MATRICES

	Factor I			Factor II			Factor III			Factor IV		
	A	N	MS	A	N	MS	A	N	MS	A	N	MS
HEAVY	−78	−25	−55	23	42	−11	−34	−31	−10	*−35	−63	−81
GOOD	*92	94	66	05	−09	40	−11	17	−49	−27	−05	−07
FAST	64	60	36	02	−30	16	26	−12	04	*65	39	83
HAPPY	*90	88	90	−06	−34	09	15	17	27	−32	07	−19
UP	45	34		−04	−13		*57	86		29	−04	
ENERGETIC	22	93		−04	−06		95	−08		07	−18	
LOOSE	44	−67		*−81	−55		−01	−09		00	−26	
STRONG	−51	74	−10	*83	41	58	−11	−25	−47	07	−35	−05
EXCITEMENT	10	52	07	−20	04	14	93	03	42	*−15	−76	−75
QUIET	19	−01		−02	−50		*−96	−47		14	45	
BLUE	−20	02	00	23	−34	−41	−68	21	−05	−28	−83	60
BAD	*−96	−98	−89	−02	13	10	11	05	03	15	−01	−38
LIGHT	89	11	77	*−27	−71	−33	29	12	10	09	64	50
DOWN	−96	−37		01	−31		12	−74		06	−23	
BLACK	*−86	−21	−91	*36	73	12	04	−49	05	14	06	07
WOMAN	62	06	47	*−39	−78	−75	−27	27	−03	−25	−16	−26
LAZY	01	−90		−20	−09		*−94	−25		−23	08	
TIGHT	−53	82		66	12		11	16		47	25	
GREEN	74	22	82	−27	18	−28	04	−23	16	−49	−86	04
NOISY	−00	−15		−13	25		*93	44		−18	−63	
GREY	10	−22		*−49	−92		−77	−08		21	02	
SLOW	−24	09	−68	−17	−81	−23	*−82	−23	−25	−37	12	−59
WHITE	*87	53	81	−09	−52	05	−21	00	−28	18	48	32
CALM	50	−04	53	−07	−83	−11	*−84	−23	−64	−04	17	19
MAN	−41	47	−18	*68	63	93	45	−12	−02	12	−34	−07
YELLOW	71	40	75	*−60	−75	−58	29	04	19	−05	−47	−18
WEAK	28	−62	28	*−90	−59	−40	−16	04	71	17	21	08
SAD	*−65	−81	−99	−19	−18	01	−69	−05	−01	06	15	−01

* Concepts tending to load in same way on a factor for all groups.

the subjects in different language/culture groups be shown to be using the same underlying factors in making their synesthetic judgments, and do these factors correspond in any identifiable way with those obtained by Suci with the usual semantic differential? Both concept and visual-alternative matrices (separately) for Anglos, Navajos, and Mexican-Spanish were factored by Thurstone's Centroid Method (with communalities fixed at 1.00), rotated by the Quartimax Method, and related across groups by the Wrigley-Neuhaus Coefficient of Factorial Similarity.[4] The Japanese data, collected later, were not submitted to factor analysis; however, both the high over-all correlations of Japanese with Anglo matrices (Table 7) and inspection of the Japanese correlations (knowing the Anglo factors—see below) indicate that the Japanese factors would correspond closely to the Anglo factors. All correlational and factorial work was done on ILLIAC.

Concept matrices. It will be recalled that many of the polar scale terms used by Suci (e.g., *heavy-light*, *good-bad*, *happy-sad*, *fast-slow*, *strong-weak*) were included *as verbal concepts* in the present synesthesia study. If a stable semantic frame of reference is operating, one would expect the same or highly similar factors to appear in both cases. The first four factors in the unrotated Centroid Analysis accounted for 89% of the total variance for Anglos, 82% for Navajos, and 84% for Mexican-Spanish. Table 8 gives the rotated factor loadings obtained by Quartimax. Table 9 gives the indices of

[4] More detailed description of these methods and references to the original descriptions are given in Osgood et al. (1957).

factorial similarity across groups for these rotated factors; the italicized values in the diagonals index the similarity among 'corresponding' factors for the several groups.

The first factor is quite well defined as *evaluative* by the concepts which load highly on it for all three language/culture groups: GOOD, HAPPY, and WHITE on the positive pole *vs.* BAD, SAD, and BLACK on the negative pole. The difference between Anglos and Navajos in what is "evaluatively relevant" is also well defined: STRONG, TIGHT, ENERGETIC, and MAN are also positively evaluated by the Navajos (conversely for WEAK, LOOSE, and LAZY), whereas the reverse evaluation, with the exception of ENERGETIC, is given these concepts by the Anglos. The first factor for Mexican-Spanish corresponds almost perfectly with that for Anglos. The indices of factorial similarity for Factor I (A/N .60, A/MS .95, N/MS .64) are consistent with these interpretations. Inspection of the Japanese correlation matrix shows that the same concepts (with the exception of WHITE and BLACK) tend to cluster together. Suci interpreted the first factor in his analysis (of Navajo, Hopi, Zuni, and Mexican-Spanish groups) as being evaluative, the clearest scales across all groups being *good-bad*, *clean-dirty*, and *pretty-ugly*.

The second factor in our study is fairly well defined across all groups as a *potency* factor: STRONG and MAN have sizeable positive loadings for all groups and LIGHT (weight), WOMAN, YELLOW, and WEAK have sizeable negative loadings. The Japanese would probably add TIGHT *vs.* LOOSE, HEAVY *vs.* LIGHT, and BLACK *vs.* WHITE to the potency factor, this being merely suggested in Table 8. The indices of factorial similarity (A/N .69, A/MS .75, N/MS .66) confirm the above picture. Suci's second factor included *strong-weak*, *heavy-light*, *hard-soft*, and *long-short* for all groups, but also *fast-slow* and *industrious-lazy* for some groups, and he dubbed it a *dynamism* factor.

Factor III is the least generalized across groups. For Anglos it is clearly an *activity* factor: UP, ENERGETIC, EXCITEMENT, and NOISY in contrast to QUIET, LAZY, SLOW, and CALM. But this pattern does not hold up satisfactorily across the other groups, as the low indices of factorial similarity (A/N .18, A/MS .52, N/MS .25) show. Inspection of the Japanese correlation data suggests that we would have ENERGETIC, EXCITEMENT, and NOISY in contrast with SLOW, LAZY, CALM, and QUIET—which is pretty close to the Anglo pattern. Suci also had difficulty

TABLE 9

INDICES OF FACTORIAL SIMILARITY FOR CONCEPT MATRICES

	Factor I			Factor II			Factor III			Factor IV		
	A	N	MS	A	N	MS	A	N	MS	A	N	MS
FACTOR I												
A		*.60*	*.95*	−.50	−.63	−.32	.13	.43	.04	−.11	.19	.34
N			*.64*	.25	−.09	.31	.30	.07	−.24	−.15	−.16	.09
MS				−.37	−.50	−.25	.20	.40	.04	−.14	.03	.34
FACTOR II												
A					*.69*	*.75*	.04	.38	−.53	.11	.17	.04
N						*.66*	.39	−.37	.02	.12	−.27	−.17
MS							.32	.29	−.30	.32	.05	−.03
FACTOR III												
A								*.18*	*.52*	.29	−.09	.08
N									*.25*	−.07	.11	.15
MS										.09	−.10	−.08
FACTOR IV												
A											*.60*	*.54*
N												*.42*

TABLE 10
ROTATED FACTOR LOADINGS FOR VISUAL-ALTERNATIVE MATRICES

	Factor I			Factor II			Factor III			Factor IV		
	A	N	MS	A	N	MS	A	N	MS	A	N	MS
up	−30	−80	−30	−91	−28	−42	−15	−34	−68	01	16	07
vertical	−86	−08	−88	−16	−08	−13	−21	00	03	24	92	−37
homogeneous	85	08	−01	−13	−25	10	−41	−80	−87	−10	−18	−25
colorless	17	15	01	82	89	74	01	23	46	−18	15	−22
thick	−34	07	−17	83	15	26	−10	34	87	26	87	07
dark	−24	08	−06	91	34	19	−12	36	95	13	81	−03
crooked	−67	−06	16	−08	−09	−51	60	88	78	10	37	−08
hazy	15	17	11	−22	−16	−08	89	86	94	13	32	−04
blunt	94	−16		−08	23		10	67		11	−37	
rounded	90	33	75	−03	19	−28	16	81	36	18	−18	−20
diffuse	−04	−61		−93	02		14	−34		15	−48	
large	28	−28	−39	−52	−20	−13	08	−55	10	72	63	85
near	−26	61		22	−12		20	−09		78	56	

demonstrating any consistent third (or further) factor in his study. Our Factor IV displays somewhat higher indices of factorial similarity (A/N .60, A/MS .54, N/MS .42), but it is difficult to come to grips with semantically. Its positive pole is best defined by FAST, LIGHT, TIGHT, WHITE and its negative pole by HEAVY, EXCITEMENT, WOMAN. There is perhaps a Freudian flavor to this factor—something which touches on many shared myths—but the quantitative data won't bear much weight.

Visual-alternative matrices. In the unrotated factor matrices for visual dimensions, the first four factors extracted accounted for about the same proportions of total variance as in the concept factor analyses—87% for Anglos, 82% for Navajos, and 86% for Mexican-Spanish. However, the factors are not so readily interpreted (perhaps because all the variables are from a small domain). Table 10 gives the rotated loadings and Table 11 the indices of factorial similarity.

For Factor I the indices of factorial similarity only permit us to compare Anglos and Mexican-Spanish, where we seem to be dealing with a kind of *flatness* factor: *rounded* and *horizontal* best reflect the nature of the factor, with the Anglos including *homogeneous*, *straight*, and *blunt*. Inspection of the Japanese correlation matrix suggests that it would duplicate the Anglo factor closely. The Navajo Factor I is actually closer to Anglo II, as the indices of similarity show.[5]

Factor II for the visual alternatives displays the highest factorial similarities (A/N .80, A/MS .75, N/MS .75) found anywhere in this study. Perhaps surprising, since this is a visual domain, it seems to be essentially an *evaluative* factor connotatively. The defining characteristics are *colorless*, *dark*, *thick*, *down*, and *small* (vs. the opposite, favorable pole, *colorful*, *light thin*, *up*, and *large*). An interesting difference is that Anglos would include *concentrated* in the definition of the negative pole, but not Navajo. Inspection of the Japanese data confirms this picture, *concentrated* being included with *dark*, *down*, etc. as with the Anglos.

Factor III for these visual dimensions also displays a high degree of cross-cultural correspondence (A/N .75, A/MS .62, N/MS .84), but one is hard put to label it. For all groups the positive pole of the factor is characterized by *hazy*, *crooked*, *rounded*, *heterogeneous*, and *down*, and the negative pole by their opposites, *clear*, *straight*,

[5] Factors were grouped so as to maximize the sizes of the indices. In this case, Navajo Factor II has a higher similarity to Anglo II (.75) than does Navajo I (.62), and hence the apparently anomalous fact that Navajo I is assigned with Anglo I where its index is only .38.

TABLE 11

INDICES OF FACTORIAL SIMILARITY FOR VISUAL-ALTERNATIVE MATRICES

	Factor I			Factor II			Factor III			Factor IV		
	A	N	MS	A	N	MS	A	N	MS	A	N	MS
FACTOR I												
A		*.38*	*.62*	−.07	.05	.19	−.08	−.19	−.27	−.09	−.54	.07
N			*.54*	.62	.46	.44	.20	.45	.42	.20	−.16	−.40
MS				.16	.22	−.04	.35	.45	.16	−.35	−.60	−.16
FACTOR II												
A					*.80*	*.75*	−.11	.35	.59	−.17	.28	−.29
N						*.75*	−.05	.35	.46	−.23	.12	−.29
MS							−.32	−.14	.15	−.30	.02	−.15
FACTOR III												
A								*.75*	*.62*	.19	.10	.09
N									*.84*	.06	.25	−.31
MS										.35	.60	.00
FACTOR IV												
A											*.69*	*.62*
N												*.14*

angular, *homogeneous*, and *up*. One possible interpretation would be that this is a *figure vs. ground* factor. Aspects of the visual environment that stand out as figures are *clear*, *straight*, *angular*, *homogeneous*, and *up* in perceptual experience. But whereas *thickness* and *darkness* is attributed to what we have called the "background" character by the Mexican-Spanish, this is not so for the Anglos.

Factor IV seems to be what might be called a *distance* factor, but the evidence is not very impressive—particularly since there is no correspondence between the Navajo and Mexican-Spanish factors. Common to all three groups we have *large* vs. *small*; Anglos and Navajos would include *near* vs. *far*—and so would the Japanese—hence the "distance" notion; but Navajos would include *vertical-horizontal*, *thick-thin*, *dark-light*, and to some extent *concentrated-diffuse*.

Addendum: Some data on color meanings

In the course of the Southwest Project on Comparative Psycholinguistics, Dr. Susan Ervin collected semantic differential data from 21 Navajo subjects when they were judging actual samples of colored paper. These were 2¼″ by 1¾″ rectangles of dime store colored papers: red, yellow, light green, blue, purple, brown, black, and white. The red was intensely saturated and bright, the green was very pale, and the blue was about the color of the non-carbon part of carbon paper, i.e., quite deep in hue but not too heavily saturated. These color chips were rated against the 27 scales given in Table 12. These scales were *verbally* defined and they were 6-*step* scales, the middle position being eliminated to force the subjects to indicate some directional choice in each case. Subsequently, Mr. Murray Miron collected equivalent data from 24 Anglos (students at the University of Illinois), using the same color chips and the same 6-step scales. Table 12 gives the mean scale positions for each color chip on each scale, Anglo (A) and Navajo (N) results being directly compared.

Non-differentiating scales and scales used differently by Anglos and Navajos. Inspecting Table 12 we can see that there are a few scales which, either for Anglos or Navajos or both, do not differentiate among the color chips—that is, are irrelevant with respect to this stimulus domain. The scale *long-short* fails to differentiate for both Anglos and Navajos. The scales *sharp-dull*, *up-down*, and *rich-poor* fail to differentiate for the Navajos. Asterisks between Anglo and Navajo values indicate

TABLE 12

MEAN SEMANTIC PROFILES FOR COLOR CHIPS FOR ANGLOS (A) AND NAVAJOS (N)

	Red		Yellow		Green		Blue		Purple		Brown		Black		White		
	A	N	A	N	A	N	A	N	A	N	A	N	A	N	A	N	
**good	3.9	4.2	3.0	3.0	3.0	3.5	3.3	3.5	3.8	3.5	4.4	3.8	4.8	4.4	1.8*	2.9	bad
happy	2.3	3.2	2.4	2.3	2.5	2.4	4.3*	3.2	4.0*	2.6	4.6*	3.3	5.1*	3.7	2.5	2.0	sad
pretty	2.3*	4.2	2.5	2.8	2.5	3.1	3.2	3.5	3.5	3.5	4.8	3.9	4.8	4.7	2.7	2.8	ugly
sweet	2.8	3.6	2.8*	1.8	3.7	2.9	3.9*	2.7	4.3*	3.0	4.3*	2.9	4.4*	3.4	2.5	2.2	sour
clean	2.7	3.4	2.3	2.2	2.0	2.2	3.6	3.4	3.8	2.9	4.8*	3.7	5.1*	3.5	1.4	1.5	dirty
strong	1.6	2.0	4.5	4.1	4.3*	3.2	2.0	2.3	2.3	2.8	2.7	2.7	1.8	2.1	4.0	3.3	weak
hard	2.4	2.6	4.1	3.2	4.0	3.5	2.8	3.1	3.5	3.0	2.8	2.8	2.2	2.0	4.6	4.1	soft
heavy	3.3	3.5	5.3*	3.7	5.4*	3.7	2.0	2.1	2.7	2.6	1.8	2.2	1.2	1.7	5.8*	4.1	light
large	2.5	1.9	4.3*	2.9	4.0	3.7	2.8	2.4	3.0	2.7	2.4	2.4	2.3	2.0	2.7	3.6	small
thick	2.6*	4.3	5.0	4.3	4.8	4.0	2.1*	3.9	2.6*	3.9	2.0*	3.2	2.1	2.7	4.1	4.2	thin
long	3.3	2.9	3.9	3.3	3.2	3.0	3.4*	2.4	3.0	2.5	3.2	2.5	2.8	2.7	2.6	2.7	short
fast	2.0*	3.4	2.7*	4.1	2.6*	4.3	3.8	3.3	3.9	4.1	5.0	4.1	4.5	4.4	2.8*	4.1	slow
sharp	2.0	2.7	2.6	3.4	2.7	2.7	3.9	3.1	3.3	2.8	5.0*	3.3	4.5*	3.3	2.8	3.4	dull
energetic	2.1*	4.0	3.0	3.6	3.0	3.7	3.7	3.3	3.5	3.5	4.9	4.1	4.5	4.8	3.4*	2.4	lazy
hot	1.5	2.4	2.9	3.0	4.5	3.9	3.8*	2.6	3.2	2.4	3.5	3.2	3.5	2.8	4.3	4.2	cold
young	2.1*	4.0	2.5	3.4	2.0*	3.4	3.8	3.6	4.0	4.1	4.7	4.4	5.4*	4.3	2.2	2.6	old
up	3.1	2.6	2.0*	3.1	2.7	3.5	3.3	2.6	4.0*	3.0	4.6*	3.5	5.6*	2.5	1.6*	2.6	down
smooth	4.0	3.9	2.3*	3.7	2.1*	4.0	4.0	4.3	3.5	4.4	4.9*	3.9	4.4	4.2	1.7	2.5	rough
happy	4.8	5.0	2.1	2.9	2.1	2.4	4.0	3.9	4.0	4.1	4.2	3.7	5.1	5.1	2.2	2.9	angry
ripe	1.4	1.7	2.5	2.9	4.6	3.9	3.4	3.4	2.9	2.8	3.5	2.6	3.3	2.4	3.5	3.0	unripe
rich	2.7	3.0	2.5	2.9	3.0	3.4	2.9	2.6	2.1	2.9	4.3	3.4	4.0*	2.7	2.4	2.6	poor
taut	2.7	2.2	3.8	3.3	3.2	3.8	3.3	2.4	3.3	2.7	3.8	3.5	3.3	2.5	3.7	3.1	loose
straight	3.0*	4.2	3.0	3.2	3.1	3.6	2.7*	3.8	3.7	3.8	4.1	4.3	3.9	4.0	2.9	2.8	crooked
noisy	1.5*	3.2	4.0	4.0	3.9	4.8	4.3	4.1	3.7	4.0	3.9	4.0	3.7	4.3	5.1*	4.0	quiet
male	3.3	2.5	4.7*	3.6	4.7*	3.6	2.5	2.7	3.6	2.8	2.7	2.8	2.2	2.3	4.6	3.9	female
white	3.8	3.4	2.2	2.9	2.5	2.0	4.4*	3.3	4.4*	3.3	4.7*	3.5	5.9*	4.8	1.1	1.4	black
dry	3.2	4.1	3.4	3.5	3.8	4.0	4.0	4.4	3.5*	4.5	2.7	3.4	3.6	4.0	3.2	3.5	wet

* Asterisk between Anglo and Navajo values indicates a difference ≧ 1.00 scale units.

** Means were computed so that low values are toward left-hand scale term (e.g., good) and high toward right-hand term (e.g., bad).

that the difference in means is equal to or greater than 1.00 scale unit—an arbitrary unit that probably approximates a reliable difference (Osgood et al., 1957). Looking along the rows defined by scales, it can be seen that a few scales yield four or more (out of 8 possible) differences at this level: *happy-sad*, *sweet-sour*, *fast-slow*, *thick-thin*, *up-down*, and *white-black*. Whether these represent the dimensions along which Anglo/Navajo differences in color symbolism are most marked, or merely scales for which translation equivalence was not obtained, cannot be determined from these data.

Over-all similarities in color connotations. Despite the occasional differences in connotation, indicated by asterisks in Table 12, the over-all similarities between Anglos and Navajos are quite striking, as inspection reveals. A rough way of estimating the significance of this connotative agreement is as follows: In each color column in Table 12 we note each scale for which the mean deviates 1.00 units from the midpoint (3.50) *for either group*—in other words, we count only those items where at least one of our two groups shows a probably significant connotation. Then we ask: on what proportion of such items do both groups deviate in the same direction from the midpoint? For RED we find that 12/15 go the same way, for YELLOW 14/15 the same, for GREEN 12/13, for

BLUE 6/7, for PURPLE 4/5, for BROWN 11/14, for BLACK 16/19, and for WHITE 15/15 deviate in the same way from the midpoints. Since all of these proportions approximate or exceed the 5% level of significance (by a one-tail test, appropriate because we are predicting "same" rather than "different" directions), we can conclude that in general Anglos and Navajos have similar connotative profiles for these color chips. But is there anything systematic about these shared color connotations?

Connotations of brightness (WHITE vs. BLACK). Table 12 is arranged to facilitate analysis in terms of semantic factors. From inspection of the WHITE and BLACK columns, it is clear that these chips differ sharply in evaluation for both groups, WHITE being *good*, *happy*, *pretty*, *sweet*, and *clean* as compared with BLACK. Other unclassified scales reflect the same tendency, BLACK being judged *rougher*, *angrier*, and *more crooked*. Brightness also connotes potency and activity, but less consistently; BLACK tends to be the more potent and masculine but WHITE the more active and feminine. Note that *hot-cold* reverses this trend in both groups, perhaps because of the specific association of WHITE with snow and ice.

Connotations of saturation (YELLOW and GREEN vs. RED, BLUE, PURPLE, and BROWN). Yellow is the least saturated region of the spectrum and, as noted earlier, the green used in this study was a pale, pastel shade; the other colors were quite saturated. The scales which differentiate among these two sets of colors for both groups are mainly along the potency dimensions. Thus we find the more saturated colors judged to be much *stronger*, *harder*, *heavier*, *larger*, and *thicker*. The same factor is reflected in the unclassified scales which differentiate these two sets: saturated colors are *angrier*, *dirtier*, and *more masculine*, as well as being judged *rougher* by the Anglos.

Connotations of hue (following the spectrum, from RED through PURPLE). Here we get evidence of differences between Anglos and Navajos. Anglos yield a number of activity scales which follow the spectrum (*fast-slow*, *sharp-dull*, *energetic-lazy*, and *young-old*), with RED being the most active and BLUE or PURPLE being the most passive. The Navajos have *noisy-quiet*, *taut-loose*, *hot-cold*, and *ripe-unripe* follow the spectrum, but with GREEN at the peak of *cold*, *quiet*, *loose*, and *unripe*. The Anglos show the same trend for *hot-cold* and *ripe-unripe*; Anglos also include some evaluative scales as following the spectrum, *happy-sad*, *pretty-ugly*, and *sweet-sour*, RED being on the favorable sides—but this may be an effect of the particular color samples used.

Some salient differences in color connotations. In the following summary, only Anglo/Navajo differences equal to or greater than 1.00 scale unit *and* with means falling on opposite sides of the midpoint (3.50) are counted. RED is *pretty*, *young*, *energetic*, *straight*, and *thick* for the Anglos, but not for the Navajo (who characterize it particularly as *taut*, *angry*, and *masculine*).[6] YELLOW is *small*, *fast*, and *smooth* and GREEN is *weak*, *fast*, and *smooth* for Anglos, but not for Navajo (who do, however, see these pastel shades as favorable evaluatively like the Anglos). Anglos and Navajos agree perfectly on the connotative directions of WHITE, with the single exception of *fast-slow*—Anglos seeing WHITE as quite fast and the Navajos as quite slow. The remaining colors, all dark hues, are judged less favorably by the Anglos: thus, for them, BLUE is the more *sad*, *sour*, *thick*, *cold*, *straight*, and *black*; PURPLE is more *sad*, *sour*, *thick*, *down*, and *black*; BROWN is more *sad*, *sour*, and *dull*; and BLACK is more *sour*, *dull*, *down*, and *poor* than for Navajos.

DISCUSSION

This paper began with a set of questions. The first was: *can the factors obtained by Suci* (1957) *for Anglo controls and for the*

[6] In a personal communication, Professor Clyde Kluckhohn has pointed out that "color enters very prominently into Navaho ceremonialism, and therefore the reaction to such a color as red is culturally influenced in a very special way." Such ceremonial usages of color undoubtedly account for some of the differences found in this study. The fact that great similarities in color connotation are found despite such ceremonial usage may mean that the significances attached to colors in ceremonies themselves tend to follow the same rules of synesthetic translation.

Navajo (and other Southwest Indian cultures) *with verbally defined concepts and scales of a semantic differential be confirmed when his scales serve as concepts judged against 'scales' defined by purely visual alternatives*? Suci obtained two clearly identifiable factors: an evaluative factor (*pretty*, *sweet*, *good*) and a potency factor (*heavy*, *rich*, *long*, and *strong*). Our own factor analyses of the concept matrices produced two clearly defined factors also—evaluation (GOOD, HAPPY, WHITE) and potency (STRONG, TIGHT, MAN). In both cases, the third and further factors did not yield satisfactory correspondence. To this extent, then, we have confirmed the factorial generality described by Suci, in a situation where the problem of translation equivalence of scale terms is minimized.

The second question was: *can cross-language and cross-culture generality of visual-verbal synesthesia be demonstrated*? There is ample evidence for visual-verbal synesthesia within our own culture. As early as 1921, Lundholm (1921) reported data on the "feeling tones" of lines: that SAD was represented by large, downward-directed curves; that MERRY was represented by small, upward-directed lines; that GENTLE was represented by large, horizontally-directed curves, and so on. Poffenberger and Barrows (1924) confirmed and extended the relationships reported by Lundholm. Karowski, Odbert and Osgood (1942) were able to demonstrate similar relationships between word meanings and the synesthetic drawings of photistic visualizers. More recently, Scheerer and Lyons (1957), Hochberg and Brooks (1956), and McMurray (1958) have reported Western intracultural consistencies in relating line drawings and/or verbally defined visual dimensions to connotative meanings or feeling-tones. As far as I am aware, the present study is the first attempt to demonstrate that the visual-verbal synesthetic relationships characteristic of our own language/culture community are shared by peoples who speak different languages and enjoy different cultures—the Navajo, the Japanese, and the Mexican-Spanish living in the American Southwest. The over-all similarities in synesthetic tendencies across these groups are impressive—when the synesthetic relationships that are significant (.01 level) intraculturally are tested for cross-cultural agreement, approximately 90% of the relationships prove to be in the same direction. We can conclude with confidence, then, that the determinants of these synesthetic relations are shared by humans everywhere—to the extent that our sample of "everywhere" is representative.

A third question in which we were interested was: *are terms which are translation-equivalent to functional opposites in our language also functionally opposed in other language/culture groups*? The answer to this question is important for several reasons: For one thing, the semantic differential as a measuring instrument is based on the assumption that 'true' opposites do 'slice up' the semantic space into meaningfully opposed regions; for another thing, the notion of logical opposition has always had a fundamental and primitive status in Western philosophical thought—is this merely a figment of our Western language structure, or is it really fundamental to human thinking wherever it may occur? Again, to the extent that our sample of human languages and cultures is representative, the answer is clear and compelling: peoples who use different languages and have grown up in different cultural settings also utilize meaningful opposition as a pillar of their logical constructions. This conclusion was obtained under conditions in which the verbal opposites were separated in time of judgment and were determined by association with purely visual alternatives. This over-all conclusion (see Table 4) is not countered by the occasional negative instances which were found: Landar's (1957) analysis of four Navajo folk tales implies that for the Navajo the logical opposition is between *moving-stationary*, the Anglo *fast-slow* as translated being degrees of moving; the failure of *energetic-lazy* to function as an opposition for the Japanese is also tagged as a translation problem.[7] For 11 of the 12 Anglo

[7] Both Professor Seizo Ohe (at the Center for Advanced Study in the Behavioral Sciences, Stanford, 1958) and a Japanese friend agreed that the terms we used for "energetic" and "lazy" were not really opposites in their language.

oppositions described in Table 4, functional opposition is demonstrated for the other language/culture groups as well.

The fourth question concerned the connotative meanings of colors: *do colors have similar connotations for Navajos as for Anglos, and is agreement greater for actual color chips than for color words?* A separate study of the connotations of colored papers, using verbally defined 6-step scales, revealed considerable agreement in connotation. For both Anglo and Navajo groups, the brightness dimension was shown to correspond to *evaluation* (WHITE favorable, BLACK unfavorable), the saturation dimension to *potency* (saturated colors being the more potent), and, with somewhat less agreement across cultures, the hue dimension to *activity* (RED being the most active and BLUE-PURPLE the most passive). These results agree generally with those of Ross (1938) on the effects of stage lighting on moods of the audience; of Odbert, Karwoski, and Eckerson (1942) on the relations of colors to verbally defined moods; and of Wexner (1954) on similar relations. The specific correlates between color dimensions and semantic factors found in our study agree with results obtained by Tannebaum on the use of color in both abstract art and advertising (cf., Chapter 7 in Osgood et al., 1957). Our results extend these relations to at least one group differing in language and culture.[8] Because of lack of correspondence in the yardsticks used (verbally defined scales for color chips and visual alternatives for color words), direct comparisons of the connotations of actual colors with the connotations of color words were not possible. However, my own impression, based on the frequencies of disagreements on the meanings of color words [cf., Table 5 (B)] and on comparisons between word-meanings and chip-meanings where the scales were roughly similar, is that Anglos and Navajos agree more on the connotations of color chips than on the connotations of color words. For example, whereas Anglo "blue" refers to a relatively dark color close to Navy, Navajo "blue" refers to a region of the color space close to turquoise.[9]

Despite impressive over-all similarities across the language/culture groups studied in visual synesthetic tendencies, there are some clear-cut differences on particular relations [cf., Tables 3, 5 (B), 6 (B), and 12]. Such differences are open to a variety of interpretations—with no clear guideposts as to which applies in any single case. The Navajo data are somewhat less reliable throughout (which is understandable given the difficulties of communicating instructions under field conditions). This by itself could be the reason that Navajos display a smaller number of significant synesthetic tendencies (cf., Table 2), but this finding could also indicate a greater concreteness of cognitive processes in the Navajo. It may be that pre-literate peoples—peoples without a written literature—generalize less broadly through metaphor. Differences in the denotative reference of some of the verbally defined concepts (and hence, in a sense, inadequate translation) undoubtedly contribute to some of the differences described here also.

Nevertheless, there are probably some "real" cultural differences in visual synesthesia that cannot be explained away as artifactual on any of the above grounds. For one thing, it seems possible that the Navajo do not utilize an activity factor in connotative meaning to the same extent that Anglos do; in both the factor analyses of verbal concepts (Table 8) and the correlates of color dimensions (brightness, saturation, and hue), the Anglos clearly display a third, activity factor which is only suggested in the Navajo data. For another thing, we noted that both in terms of significant differences in correlations among the visual

[8] A paper by Kimura (1950) indicates that Japanese subjects relate the hue dimension to a warm-cool scale (activity), red being the most warm; and saturation to a heavy-light scale (potency), black, blue, and red being the heaviest and green, yellow, and white the lightest. These observations are consistent with our findings for Anglos and Navajos.

[9] Dr. Susan Ervin, who collected the Navajo color data says: "The two terms that are most different in reference are the words for blue and for grey. Blue refers to any color within the range we would call green-blue-purple. The best Navajo blue is a bright aqua, about the shade of the turquoise popsicles they have in the Southwest (I think expressly for Indians!)."

alternatives (Table 6) and in the over-all correlations of the visual-alternative matrices (Table 7), the Southwest-living Navajo and Mexican-Spanish agreed with each other as against the Anglos and Japanese. The shift of the Mexican-Spanish, from "allegiance" with the Anglos on concept meanings to "allegiance" with the Navajo on visual scale meanings, was particularly striking. Does this mean that growing up in the visual environment provided by the Southwest helps to organize the dimensions of the visual frame of reference in a somewhat different way than elsewhere?

This research obviously has bearing on the Sapir-Whorf "Weltanschauung" hypothesis—but in support of the converse. Most of the discussion and research relating to this hypothesis has been designed to demonstrate that differences in language do produce differences in "world view," and certainly there is both observational (Carroll, 1956) and experimental (Brown & Lenneberg, 1954; Lenneberg & Roberts, 1956) evidence for this view. The present study and others along the same line (Kumata, 1957; Kumata & Schramm, 1956; Suci, 1957; Triandis & Osgood, 1958) strongly support the position that, for certain aspects of cognitive behavior at least, "world view" may remain relatively stable despite differences in both language and culture. The apparent conflict between these two sets of findings disappears if one makes a distinction between two general classes of cognition—which, for lack of better terms, I shall call *denotative* and *connotative*. The phenomena which seem to display generality across human groups regardless of language or culture are essentially connotative—the affective "feeling tones" of meaning which contribute to synesthesia, metaphor and the like. The phenomena which display dependence upon the structure and lexical categorizing of language seem to be essentially denotative—the multitudinous and arbitrary sets of correlations between perceptual events and linguistic events (i.e., the "rules of usage" of any language code). The distinction I am making has the status of an hypothesis, not a conclusion, but the meager evidence available seems to be consistent with it.

Finally, we may inquire into the reasons behind similarities in connotative systems despite language/culture differences. First, by virtue of being members of the human species, people are equipped biologically to react to situations in certain similar ways—with autonomic, emotional reactions to rewarding and punishing situations (evaluation), with strong or weak muscular tension to things offering great or little resistances (potency), and so on—and hence they can form connotative significances for perceived objects and their linguistic signs varying along the same basic dimensions. Such connotative reactions enter into a wide variety of meaningful situations, are therefore broadly generalized, and provide a basis for synesthetic and metaphorical transpositions. Beyond this shared connotative framework, there are many specific relations between human organisms and their generally similar environments whose stability can be the basis for synesthetic and metaphorical translations. These may be either innate to the species or developed by learning under similar conditions. An example of the former (innate) basis may be the common association of the red end of the spectrum with warmth and activity and the blue end with coldness and passivity. An example of the latter (acquired) basis may be the common association of visually large with auditorily loud—it is simply a characteristic of the physical world that as any noise-producing object approaches or is approached, increases in visual angle are correlated with increases in loudness. These "homotropisms" and experiential contingencies may be expressed in language but are independent of the structure of any particular language.

REFERENCES

Brown, R. W. & Lenneberg, E. H. A study in language and cognition. *J. abnorm. soc. Psychol.*, 1954, 49, 454–462.

Carroll, J. B. (Ed.). *Language, thought and reality: Selected writings of Benjamin Lee Whorf.* New York: Wiley, 1956.

Casagrande, J. B. The Southwest Project in Comparative Psycholinguistics: A progress report. Social Science Research Council, 1956, Item 10, 41–45.

Hochberg, J. & Brooks, V. An item analysis of physiognomic connotation. Unpublished study, privately distributed, 1956.

Karwoski, T. F., Odbert, H. S., & Osgood, C. E. Studies in synesthetic thinking: II. The role of form in visual responses to music. *J. gen. Psychol.*, 1942, 26, 199–222.

Kimura, T. Apparent warmth and heaviness of colours. *Japanese J. Psychol.*, 1950, 20, 33–36.

Kumata, H. A factor analytic investigation of the generality of semantic structure across two selected cultures. Unpublished doctoral dissertation, University of Illinois, 1957.

Kumata, H. & Schramm, W. A pilot study of cross-cultural methodology. *Publ. Opin. Quart.*, 1956, 20, 229–237.

Landar, H. J. Four Navajo summer tales. Report of the Southwest Project in Comparative Psycholinguistics, 1957.

Lenneberg, E. H. & Roberts, J. M. The language of experience. *Suppl. int. J. Amer. Linguistics*, 1956, 22, 33.

Lundholm, H. The affective tone of lines: Experimental researches. *Psychol. Rev.*, 1921, 28, 43–60.

McMurray, G. A. A study of "fittingness" of signs to words by means of the semantic differential. *J. exp. Psychol.*, 1958, 56, 310–312.

Odbert, H. S., Karwoski, T. F., & Eckerson, A. B. Studies in synesthetic thinking: I. Musical and verbal association of color and mood. *J. gen. Psychol.*, 1942, 26, 153–173.

Osgood, C. E., Suci, G. J. & Tannenbaum, P. H. *The measurement of meaning*. Urbana: The Univ. of Illinois Press, 1957.

Poffenberger, A. T. & Barrows, B. E. The feeling value of lines. *J. appl. Psychol.*, 1924, 8, 187–205.

Ross, R. T. Studies in the psychology of the theatre. *Psychol. Record*, 1938, 2, 127–190.

Scheerer, M. & Lyons, J. Line drawings and matching responses to words. *J. Pers.*, 1957, 25, 251–273.

Suci, G. J. An investigation of the similarity between the semantic spaces of five different cultures. Report for the Southwest Project in Comparative Psycholinguistics, 1957.

Triandis, H. C. & Osgood, C. E. A comparative factorial analysis of semantic structures in monolingual Greek and American college students. *J. abnorm. soc. Psychol.*, 1958, 57, 187–196.

Wexner, L. B. The degree to which colors (hues) are associated with mood-tones. *J. appl. Psychol.*, 1954, 38, 432–435.

(Manuscript received May 9, 1959)

The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed, the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back. I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas. Not, indeed, immediately, but after a certain interval; for in the field of economic and political philosophy there are not many who are influenced by new theories after they are twenty-five or thirty years of age, so that the ideas which civil servants and politicians and even agitators apply to current events are not likely to be the newest. But, soon or late, it is ideas, not vested interests, which are dangerous for good or evil.

J. M. KEYNES, *The General Theory of Employment, Interest, and Money*

COMPUTERS IN BEHAVIORAL SCIENCE

THE IMPACT OF COMPUTERS ON PSYCHOLOGICAL RESEARCH

A symposium jointly sponsored by Divisions 5 and 12 of the American Psychological Association, and presented at that society's meeting September 7, 1959, Cincinnati, Ohio. Chairman: Harry H. Harman, System Development Corporation.

Previous American Psychological Association sponsored symposia on digital computers dealt with the outlook for the future, and the anticipated impact of the computer on psychologists and their research. Is computer use as widespread as was predicted a few years ago, and has its utilization contributed either to the solution of significant psychological issues or the discovery of new problem areas? This symposium was organized by Philip A. Smith of System Development Corporation to explore the ways in which the computer has fulfilled its early promise, and to answering the following questions: (a) Have psychologists utilized the computer chiefly as an easier, faster means of handling routine statistical chores, or have they found it possible to develop novel methods and broader concepts of research? (b) Has the computer served to bring clinical psychologists and their theories any closer to other scientists and their theoretical or methodological formulations? Has the computer successfully aided the exchange of concepts and ideas?

Medical Diagnosis By Computer: Recent Attempts and Outlook for the Future,

*Steven G. Vandenberg, Mental Health Research Institute, The University of Michigan**

While electronic computers are being used all over the country for business and scientific computations—including many social science computations—there are still relatively few non-computational applications of computers, such as in military inventory control, airplane ticket reservations, and in banking. The remaining projects consist mainly of small scale demonstrations or feasibility studies of problem solving, learning, pattern perception, foreign language translation, information retrieval, medical diagnosis, etc.

My remarks today will be limited to the role of computers in medical diagnosis. There has been enough interest in this area to have led to a conference on diagnostic data processing at the Rockefeller Institute for Medical Research (1959), while various papers on related topics have been presented in the last year or so at several computer conferences. The Russian Soviet Fleet Newspaper carried a story about a diagnostic computer according to a recent AP release.

Rather than give you a complete rundown or annotated list of references of the work done in Russia, France, England, the Netherlands, and the USA, let us take a minute or two to look at a few. Three species can be distinguished and if you look closely it seems that all their possible hybrids are forthcoming. We might name the three main types *simulation*, *statistical analysis*, and *logical diagnosis*.

Simulation. Simulation is being used for instance in vectorspatial electrocardiography (Simonson & Schmitt, 1955) and in neurophysiology. Usually a greatly simplified model of the system under study is programmed for a digital computer or—

* The literature search for this paper was supported by the Schizophrenia and Psychopharmacology Study, a joint research project of The University of Michigan and Ypsilanti State Hospital, (Ralph W. Gerard, Principal Investigator), U. S. P. H. S. MY 1972 C.

more often—the proper electrical circuits are built into an analog computer and the behavior of this model is observed continuously or at certain critical moments.

Most of the phenomena of interest to psychologists are very complex and have so far not been attacked with computers. Exceptions are learning, pattern perception, and problem solving, about which others will speak.

Statistical analysis. Examples of the statistical work are the auto and cross-correlational analysis of EEG and polygraph records, which Dr. Ax of the Lafayette Clinic in Detroit is working on. In the Schizophrenia Project of the University of Michigan and the Ypsilanti State Hospital, we are correlating a large number of biochemical, physiological, and psychological variables with the ward behavior, social history, and psychiatric interview items of schizophrenic and other patients who are brought into our research ward for a nine week period. Without the aid of computers, the cost of such large scale statistical analyses would be prohibitive. But you are all familiar with the wide use of computers for this type of work, so I will not take up time talking about this.

Logical diagnosis. The third type of computer analysis, the logical analysis of diagnosis, is perhaps the most exciting. A paper by Ledley and Lusted (1959) gives a comprehensive description of the ways in which symbolic logic, probability theory, and value theory can be used to formalize the diagnostic process sufficiently so that a computer program can be written which goes through this process step by step. If enough knowledge exists about a particular area, an incidence-of-symptoms table somewhat like a logical truth table may be constructed with diseases and symptoms defining the rows and columns; and with zeroes, questions marks, and ones in the cells to represent the presence or absence of each symptom in the various diseases. Refinements can be introduced by using the probability of the occurrence of each symptom in each disease, as well as by considering to what extent the presence of the several symptoms may be regarded as contributing information which is independent both logically and statistically. Next, the likelihood of various diseases can be dealt with by considering the subject's age, sex, previous history, exposure to specific climatic or occupational conditions such as stay in the tropics, radiation, harmful metals, as well as hereditary diseases in blood relatives, the season of the year, and the location of the diagnostic center.

When finally a decision has to be made regarding treatment, the paper by Ledley and Lusted shows how von Neumann's theory of games may be used to determine the optimum strategy in the game between physician and nature, provided that the positive and negative values attached to the various 'pay-offs'—representing the values associated with the expected outcomes of the acceptance or rejection of the various treatments—have been decided upon. These values are usually based on highly personal decisions of the physician, or the physician in consultation with the patient or relatives of the patient, but may sometimes be regulated by religious ethics or by military code.

The more advanced and precise the knowledge is in the particular area, the easier it is to construct the table of diseases and symptoms. Of course, in the ideal situation where there exists a complete one-to-one relationship between diagnostic test, symptom, and disease, a computer program is hardly necessary unless computer time becomes cheaper than the time of human clerks. It is particularly when the relationships are more complex that aid from computers may be valuable, provided that every relationship can be spelled out in detail.

It is unfortunately true that in the diagnosis of mental diseases there exists as yet no such orderly system of clearly understood relationships. Attempts to develop a more explicit system are under way. The search for objective diagnostic tests as well as the construction of the various psychiatric rating scales constitute important steps in this direction. The more factual the items in these rating scales, the more interesting it will be to correlate such items with various diagnostic tests. It may well be that the

psychiatric categories are too heterogeneous and that the choice of treatment and prognosis will have to be based on new and finer groupings of patients arising out of the constant amassing and cross-correlation of vast masses of data. Before this can be done effectively, there will have to be much more uniformity in the way in which history and mental status information is obtained, recorded, and stored. It seems not unlikely that some mental hospitals will acquire punched card systems which would allow routine periodic analyses of such data. Nor does it seem unlikely that a number of such hospitals may develop a common system so that the problem of small samples could be avoided.

Whether or not the adoption of such common systems of detailed punched card records will lead in time to the use of computers on anything like a routine basis is uncertain. Psychiatric diagnosis by computer cannot be developed without going through this more prosaic step. At any rate, it seems fairly certain that diagnosis with the aid of a computer will be far easier to work out for various medical specialties other than psychiatry—even though its potential contribution might be greater there—because uniformity of diagnostic procedures will be so much more difficult to achieve. Curiously enough, many of the engineering problems one would encounter in constructing a large scale diagnostic computer are being solved, so that it is possible to think of developments on a par with the most imaginative science fiction stories (Vandenberg, 1956). The time may come when a network of computers is tied in with various record keeping agencies such as the Bureau of the Census, hospitals, social service bureaus, income tax bureaus, police departments, insurance companies, banks, schools, and military and civilian employers to keep cumulative records for every person on their rolls. (Newcombe, Kennedy, Axford, & James, 1959). Such records can be displayed on picture tubes and photographed at enormous speeds. Similarly, documents can be read at increasing speeds. Direct verbal communication with computers is being developed so that soon, for certain purposes, one can converse with the computers without a human intermediary punching tapes, cards, or buttons.

Potential effects

There is a certain fascination in speculating what the psychological effect of such arrangements might be. In the case of direct communication with the computer, the already present tendency to personify the equipment will undoubtedly grow and it will often be forgotten that the "behavior" of the machine has been planned in every detail by a staff of programmers and executive policy makers.

While direct communication with the computer may lead to unpleasant psychological attitudes and feelings, it should be remembered that such use of computers would at least be known to the individuals communicating with the machine. It is much more likely actually that there will be a good deal of secret, non-public, or at least not widely known uses of computers without the direct cooperation or knowledge of the individual about whom facts are being processed.

There are some very interesting legal and political battles ahead of us when decisions will have to be made about the availability of records for such purposes. The question who may have access to the various types of records will come up many times before clear-cut policies can be worked out. Some of the problems concerning the use of wire-tapping, and lie detectors as well as the right of privileged communication will provide legal analogies and precedents. The risk of increased regimentation and red tape will be great, perhaps great enough to off-set any gain in efficiency and knowledge which might be won.

Perhaps it might be possible to limit such systems to include only those persons who wish and who are likely to benefit clearly, giving each and every one a choice in the matter. Insurance companies might move in this direction or larger business concerns might develop such systems for employees willing to cooperate. Many safeguards will have to be developed to prevent the occasional injustice through inaccuracies. Al-

ready we have read in the newspapers about a woman who was kept imprisoned for some time because of an error made by punched card equipment or operator.

I, for one, believe that at least as many errors of judgment as well as errors due to inaccuracies are made today by the innumerable human records keepers and officials as would be made by a computerized system. At least errors of judgment might be minimized there and the element of personal bias eliminated. It may be a matter of taste whether it is more aggravating to be unfairly treated by a machine or by a human being. Of course, it may be that for political, economical, and psychological reasons it will be advantageous to keep on employing human intermediaries so that the public never deals directly with the computer. Clerks could pass information to the computer by teletype-writers and information could come back to them to be given out in the clerk's own words and voice.

Perhaps fears of unfavorable attitudes are unwarranted. In the world of tomorrow, everyone may be sufficiently exposed to certain types of electronic devices in his office or factory, in the library or in his military service so that more indifferent or even favorable attitudes may develop. One thing is certain: there is a need for studies on attitudes towards computers. Already there have been many cartoons and jokes about computers. While they would not form a representative sample they might define the range of attitudes. The treatment of computers in science fiction stories could form another source of opinions and feelings. Such more or less "literary" analysis should be supplemented by data from polls and from experiments in which the effects of education and pertinent information on attitudes toward computers can be assessed.

It is likely that differences will be found between the attitudes of those who might imagine themselves someday to be personally involved either as potential "cases on record" in such a system, or as users or as operators of such a system, and those who see such events as less likely to concern themselves directly. Nevertheless, the information from such studies would not only be of considerable interest in itself, but it could serve to guide the development of increased use of computers in the public domain for social, medical, and other personal benefits to mankind.

Suggested research

After this look into the near future, I would like to return to the present for a few more suggestions for research. Whether or not computers will ever be used to help in psychiatric diagnosis, it will be of enormous value if we could accumulate the kind of information which would be necessary for such a system. The kind of information needed would far exceed what currently is being collected by the Federal Government, or by any state, and should include the incidence of a number of details about history and present mental status and their interrelationships as well as their correlations with clinical tests, treatment results, etc. Studies by Morton Kramer and Hyman Goldstein of the Biometrics Branch of the National Institute of Mental Health indicate that some of these relationships may be quite complex. Next, it would be necessary to write out in detail a diagnostic manual, which would permit the construction of a complete logical tree such as would be needed to construct the computer program. While one may hope that the data used would be gradually limited to objective measures and factual items, there is no reason why they could not include intuitive judgments of ego-strength, paranoia or flatness of affect, provided such items are described in enough detail to assure a certain degree of uniformity in the judgments.

The human diagnostician might be a part of the system and could be asked to provide the eyes and the intuition which the machine would lack forever or at least for a long time to come. Studies of this kind are urgently needed to bridge the gap between, or rather to combine, clinical and actuarial prediction.

While the development and acceptance of computer aided diagnosis in other areas could provide considerable impetus for the construction of such a system, there is no need to wait for this uncertain event. In

fact, a diagnostic manual a little bit like this has been constructed by D. H. Stott and Miss E. G. Sykes of the University of Bristol in England. The Bristol Social Adjustment Guides (1958) consist of a set of forms for the systematic recording of a child's behavior which can be analyzed to provide diagnostic information and suggestions for treatment. Perhaps it will be possible to construct a manual which would eliminate the necessity of asking everyone the same questions. Once it is known that the subject is not married or an only child or paranoid, one could go to a certain section of the manual constructed for such cases and progressively narrow down the number of possibilities. There is a popular book which approached best seller status some years ago in which this method was applied (Loewenstein & Gerhardi, 1942). Since it was not based on statistical analyses it was not very adequate but the principle of asking only pertinent questions was present.

Before closing, we should consider the question whether completely automatic diagnosis without human intermediaries will ever be possible. Clinicians will say: No! A machine will never be able to develop empathy. The computer enthusiast will say: But given enough instances a heuristic program stored for such eventualities could develop or learn a reasonable facsimile. It seems best to straddle the fence on this one.

REFERENCES

Ax, A. F. A data processing system for multiple physiological recording. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Baruch, J. J. Doctor-machine symbiosis. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Beckett, P. G. S. Computers and clinical psychiatry. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Brannick, L. J. An analog approach to computer diagnosis. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Brodman, K., van Woerkom, A. J., Erdmann, A. M., & Goldstein, L. S. Interpretation of symptoms with a data-processing machine. *AMA Arch. of Int. Med.*, 1959, 103, 776–782.

Burch, N., Silverman, A. & Greiner, T. H. New approach to theory and automatic analysis of EEG. *Proc. Soc. EEG Clin. Neurophysiol.*, 1956, 8, 157.

Davies, D. W. Mechanization of thought processes, *Nature*, 1959, 163, 225–226.

Doyle, L. Programmed interpretation of text as a basis for information retrieval systems. Paper presented at the *West. Joint Comp. Conf.*, San Francisco, March 3–5, 1959.

Dunn, H. L. Electronic data processing of national vital statistics. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Eden, M. Storage and retrieval of the results of clinical research, *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Kelly, W. A. Possible application of EDP in daily practice. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Ledley, R. S., & Lusted, L. B. Reasoning foundations of medical diagnosis. *Science*, 1959, 130, 9–22.

Loewenstein, L., & Gerhardi, W. *Meet yourself as you really are*. New York: Penguin Books, 1942.

Menelly, G. R. Hurdles to surmount in a program of medical diagnosis by EDP. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Newcombe, H. B., Kennedy, J. M., Axford, S. J., & James, A. P. Automatic linkage of vital records. *Science*, 1959, 130, 954–965.

Paycha, F. Mémoire Diagnostique. *Montpellier Méd.*, 1955, 47, 588. (a)

Paycha, F. Mémoire electro-mechanique de diagnostic. *La Presse Médicale*, 1955, 63, 1151. (b)

Paycha, F. Critique scientifique des démarches intellectuelles du diagnostic. *La Presse Médicale*, 1955, 63, 1753–1756. (c)

Paycha, F. Medical diagnosis and cybernetics. *Symp. on the Mechanization of Thought Processes*, National Physical Laboratory, Middlesex, England, H. M. S. O., 1958.

Simonson, E. & Schmitt, O. H. Present status of vectorcardiography. *AMA Arch. Int. Med.*, 1955, 96, 574–590.

Stott, D. H. The social adjustment of children. *Manual to the Bristol Social Adjustment Guide*. London: University of London Press, 1958.

Tanimoto, T. IBM 704 medical diagnosis program. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Taylor, R. Major problems in the use of computing machines. *Rockefeller Inst. Conf. on Diagnostic Data Processing*, 1959.

Vandenberg, S. G., Great Expectations or the future of psychology (as seen in science fiction). *Amer. Psychol.*, 1956, 11, 339–342.

Weinrauch, H. & Hetherington, A. W. Computers in medicine and biology. *JAMA*, 1959, 169, 240–245.

Can the Computer Supplant the Clinician?, *Wayne H. Holtzman, The University of Texas*

Abstract.[1] A machine cannot interact with the subject as the clinician does, but machines could help (a) in collecting routine information such as biographic details or MMPI test responses; (b) in processing test protocols; and (c) in making certain types of interpretations based on actuarial predictions. As an example of machine processed protocols, the Holtzman Inkblot Test is discussed. An IBM 650 program for configural scoring has been written by E. Moseley and G. McCollough. The unique contributions of the clinician are "his experience and intuition, qualities lacking in the computer except in a trivial sense."

A Computer As An Experimental Laboratory Machine for Research on Automated Teaching Procedures, *Harry F. Silberman, System Development Corporation*

This will be a description of a plan to use a computer as a research tool for teaching students. The computer will be used for studying different teaching procedures without having to build a new machine for each procedure to be studied. Let me mention that the term program refers to a computer program and the term item set refers to a group of subject matter questions.

It is being shown with increasing frequency that simple reinforcement principles can be used to promote human verbal learning when applied in teaching machines. By teaching machine is meant a device that presents problem materials to a student, requires the student to respond to these materials, and provides the student with knowledge of results. Teaching machines vary in complexity from a simple paper device to a digital computer. In a pilot study we conducted recently, an attempt was made to study various features of such devices by using a human experimenter to simulate the control mechanism of the machine. It soon became apparent that manual simulation of some aspects of a teaching machine was not suitable. Intricate methods of sequencing subject materials had to be reduced to simplified procedures due to experimenter limitations. These procedures were poor substitutes for the originally planned treatment conditions, so we decided to use a computer for subsequent experiments. That is, a computer would be programmed to act as a teaching machine. Nancy Anderson at IBM has already demonstrated the feasibility of using a computer to teach binary arithmetic (Rath, Anderson, & Brainerd, 1959).

As to the goal of our research with this tool, we feel that machine responsiveness to the student is a variable that has not received enough emphasis in existing teaching machines, so this research is directed primarily toward an appraisal of the effect of dynamic adaptation of instructional procedures to the individual student. Our problem is to determine what are the salient aspects of student behavior to which the machine should be responsive and what transformations occur between the student behavior and the machine outputs in order to optimize learning.

Potential capabilities of this research tool are quite extensive. The combined effect of manipulating a fairly large number of variables can be studied. The machine will be responsive to different types of student behavior such as response latency, errors, questions, commands, and self evaluation. The machine will interact with the student and attempt to minimize both the number of student errors and the time required to complete a lesson. This might be done by appropriate modification of the rate, sequence, type, and amount of information presented to the student in response to his behavior.

To illustrate the operation of the machine, I might describe a possible design for the computer program being formulated. First let's define four terms: basic series, item type, subject mastery index, and constraint. A

[1] The full text of this paper by Dr. Holtzman appears in the *Journal of Clinical Psychology*, 1960, 16, 119–122.

basic series is any prespecified sequence of subject matter items that constitute a teaching approach. One basic series starts with specific facts and works inductively up a tree of examples ending in a general principle. Another basic series starts with the principle and ends with specific examples of the principle. Second, item types. Subject matter items may be classified according to functions such as introduction items or review items. Third, the subject mastery index is a value which provides a method of ordering concepts or teaching subgoals into levels of difficulty. A student's success or failure on a set of subject matter items covering a particular concept or subgoal changes the value of a counter in a given storage location corresponding to that subgoal. Constraints are rules that are used in selecting subject matter items for a particular subgoal or concept. Constraints might, for example, be imposed on the number of items of each type that are selected for a particular subgoal, or the sequence in which the item types are selected.

With these terms the functional steps of problem selection might be programmed as follows: Start presenting items in the basic series, evaluating the student's response, and giving him feedback on his performance. If a certain number of items are missed on a part of the series, start the student on a remedial detour. Establish which subgoals or concepts are relevant to this portion of the basic series and sense the subject mastery index of these concepts to determine which concept to cover first. Then select the items covering this concept according to the constraints imposed on item selection. When this subgoal is covered, pick up the next subgoal and then the next until the relevant subgoals are completed and then return to the basic series.

If a certain number of these remedial detours occur or if an excessive amount of time is spent in a detour, or if the student tells the machine that he is "lost" (by pressing a certain key on the input keyboard), then several things can be tried. An alternate set of constraints can be used during subsequent remedial detours. If this doesn't work, try an alternate basic series. If this doesn't help, start repeating items and then give up on the student and tell him to take a ten minute break. On the other hand, if the student was doing well on the original basic series, sense the subject mastery index and decide either to skip the rest of the items in a particular concept and go to the next concept, or just continue in the basic series until the lesson is completed.

This represents only one of several approaches which might be programmed for experimental purposes. There are, of course, many problems associated with such an approach. One can easily overestimate the extent to which the teaching process can be automated and underestimate the difficulty of the programming task. The output mechanism is also a potential source of trouble. We are considering a random access external slide projection mechanism as an output unit. Each slide would contain one subject matter item. Preparation of these items, of course, is one of the most important problems we have.

Probably the difficulty of accomplishing the programming is much less of a problem than is the determination of what to put into the program model. Potentially significant hypotheses are not readily apparent. One method of obtaining hypotheses is to choose a particular theoretical learning model and make the tutoring program the complement of this model. Another method is to obtain a verbal description from a specific tutor on how he teaches and try to simulate this description.

Possible outcomes of using the computer as an experimental laboratory machine include information on how production devices ought to be modified, or how more efficient sets of items can be constructed for conventional devices. But most important, an answer should be obtained to the question of whether or not machine responsiveness contributes significantly to effective teaching.

REFERENCE

Rath, G. J., Anderson, Nancy S., & Brainerd, R. C. The IBM research center teaching machine project. In E. Galanter, (Ed.), *Automatic Teaching: The State of the Art*. New York: John Wiley & Sons, 1959.

Intelligence in Computers: The Psychology of Perception in People and in Machines. *Leonard Uhr, Mental Health Research Institute, The University of Michigan.*

Simon and Newell (1958) have asserted that "within ten years . . . the digital computer . . . will be the world's chess champion, . . . will discover and prove an important new mathematical theorem, . . . will write music . . . possessing considerable aesthetic value." . . . and that most psychological theories will be computer programs.

This is probably even more startling to those scientists who are working with computers than to the layman. (And, in fact, most computer engineers appear to be tolerantly skeptical.) For the computer is, as well as an enormously powerful machine, an enormously stupid one. It is really nothing more than a set of switches—an abacus gone mad with size and speed. But Turing and McCulloch and Pitts have shown that it is capable of doing anything that its functionally equivalent duplicates—logic (Turing, 1936) and brains (McCulloch & Pitts, 1943)— are capable of doing. On the other hand, there is no algorithm (that is, decision procedure) for most interesting problems in mathematics, much less the sciences and arts. So heuristic methods, and especially those of the brain, must be discovered. Any scientist who assumes that non-describable, mystical entities do *not* play a part in the functioning of "mind" is *forced* to accept that the computer and mind are (potentially) functionally equivalent, no matter what his position as to consciousness, mind-body, and similar problems. Programming intelligence into the machine black box should be easier than determining it in the human, and can't help but be relevant to the latter problem.

In contrast to this potential power, the actual non-algorithmic use of the computer today is as a permanently arrested embryo; or, at best, an idiot savant. The complicated things that it can do simply, or that happen to be built in (for example, arithmetic, calculus, remembering), it does phenomenally well. Much simpler and/or more interesting things (information retrieval, recognition, decision making, creative thinking) it can scarcely do at all. Yet it is capable of all these—exactly as capable as the human brain—and capable of using the brain's (or even better) methods.

The computer's potential abilities can't really be appreciated until they are contrasted with its basic simplicity. The simplest way of looking at a computer would be to compare it to several strings of beads. Take one string of beads and you have the abacus. The string of beads now can be used for any mathematical operation, based obviously on counting. Take a second string of beads and use this to store information. Use notched beads placed in two positions: notch up or notch down. Now our abacus operator uses his first string to perform operations on information that he notices on the second string. And when the operator gets answers he puts them onto the second string for his input-output colleague to read off. This is quite an exact description of the computer.

Increase the size of the first string, actually not very much, and build some fancy short-cuts into it, fancy only in the way the modern day abacus is fancy. Increase the size of the second string enormously (for example, the IBM 650 has 2,000 of these strings, each string about 35 beads long, arranged on a magnetic memory drum that whirls around at the speed of 200 revolutions per second. The whirling is only a method to allow access to any one of the strings in as short a period of time as possible). Replace our abacus operator by electronic switches. Now instead of moving at best a few beads per second, we are limited only by the speed of light. (Computers in operation today are able to make several parallel moves at speeds of around a million per second.) Further, logical arithmetic operations can be done in parallel. This amounts to a very simple bartering of components; that is, increasing the number of logical circuits to get speed. Here, certainly, is an interesting analogy to the human with his millions of input cones in the eye and hundreds of thousands of parallel transmitting neurons in the optic nerve. Von Neumann

made the provocative point that the brain appears to have an entirely new method for deductive logic—one that seems to involve much less "depth" in the number of steps (neural layers) necessary to reach a conclusion (1958).

Very interesting further technological advances are being perfected in the laboratory. People are already writing papers worrying about the speed of light as a limiting factor, or suggesting the use of individual free electrons as storage units. Here is a beautifully simple gadget of exactly the same sort of simplicity as the brain.

The typical small computer (IBM 650) today has a memory storage capacity of about 100,000 bits. The large computer (IBM 704) has a fast access storage capacity of several million bits, plus many millions more on tape or discs. Disc memories are modelled on the juke box. They now store up to a billion bits of information with average access time of less than a second. The interesting thing here is that the slow access time memories are now reaching the capacity that has been estimated for the human brain (10^{10}). (The high speed logical operations and fast access memories have always been a great deal faster than the brain.)

Ten years ago the computer was an interesting, but hopelessly oversimplified, analogy to what the brain might be. It is today identical in certain aspects (switching times less than a microsecond; memory storages approaching a billion bits). It promises, within the next ten years, to actually achieve —in potential—all the powers of the human brain.

There are several major psychological functions for which machines must be developed, for which, today, the tentative foundations of a "psychology of machines" are being laid. Probably the most interesting work at the moment is going on in cognition—in theorem-proving, problem-solving and game-playing machines (Newell, Shaw, & Simon, 1959; Gelernter, 1959; Gelernter & Rochester, 1958). Most people appear to feel that, although one of the least advanced, the field of greatest ultimate importance is learning—if only, because as with people, it will be too complicated to write specifications for intelligent machines (Friedberg, 1958; Samuel, 1959; Kilburn, Grimsdale & Sumner, 1959).

PERCEPTION MACHINES

The greatest amount of work has been done in the theory and design of machines that perceive. Yet relatively little use has been made of existing knowledge of the human perceiver. But the human machine is a "perceiver" that far surpasses any proposed human-made machine. Its very existence is a living demonstration of what can be done. It seems especially promising to design machines as humans appear to be designed, in areas where humans do better. And the recasting of human specifications (that is, of the science of perception, and the related fields of psychology) into machine-compatible specifications could give clarity and rigor to the language and concepts of psychology, and open the possibility of man-machine comparisons, cross-simulations, and substitute experiments. (That is, the use of a machine as subject when it is desirable to vary a factor that cannot be tampered with in people—for example, previous experience. Or the use of a person in the same way—for example, to get phenomenological correlates of behavior.)

There is a critical need for better perceptual mechanisms in machines. The sensory mechanism problem is probably close to solution, with a rapid advance in the art of flying-spot scanners and contour trackers, photo-electric cell arrays, and similar optical-mechanical-electronic gadgets. But the true perceptual processes, the information-processing and recognizing of the inputs sensed, are still at a primitive stage.

Most information-processing uses of computers boil down to manipulations of alpha-numeric symbols—numbers or words. These symbols, manipulated by the millions per second, must first be prepared by a human being at a keyboard, at the rate of 10 bits per second. There are potential uses of the computer of great importance—for example, automatic storing, indexing and dissemination of information in books, journals and

documents, abstracting, and translating—that wait chiefly on an efficient input device. In other fields, automatic readers are vitally needed for things like radar, bubble chambers, star maps, aerial photos, EEG's, Morse code, verbal commands, Finnegans Wake.

Local processing methods

At the simple-minded extreme are machines that process specially prepared bank checks (Broido, 1958). Most merely sense dots and matrix cells, but placed in special arrangements so that they compose into letters. That is, they sense specific atomic elements in particular positions of an externally superimposed matrix, and merely match these up against successive patterns or templates until congruents are found. Relatively successful as this has been when given the flexibility of sub-part matching (Shepard & Heasly, 1955; Heasly, 1959), a small ability to wobble or wiggle (Bailey & Norrie, 1957) plus tolerances for noise, both its ultimate power and psychological interest can be questioned. Methods of this sort will not work to the extent that members of a common class can vary (say from an open-looped squared 4 to a closed, curved 4, when similarly cross-varying 9's exist). And they become excessively complicated when special recognition programs or sequences have to be built in for each variation that might occur on the meeting, not-quite-meeting, crossing, or slanting between two line segments, for example from Roman to Italic face.

On the other hand, simple-minded and fallible as this type of template or atomistic-matching approach may be, it is certainly a model for perception. In fact it is close to the classical associationist model, with occasional modulations of sophistication. Here, then, is an interesting operational test for this sort of approach—demonstrating both a surprising power and, probably, fundamental ultimate limitations.

The name "analytic" has been given the methods for perception that show, in the opinion of most people, the most immediate promise. These might best be described as the methods for abstracting from combinations of basic units particular features or qualities of importance, for identifying rather more abstract and complex things like edges, ends, curves, angles, and slopes—in general, for analyzing rather than matching. These approaches suggest things like synaptic summation, cortical projection areas, qualities, abstraction and classification, even mentalistic concepts like ideas and ideals.

A number of very interesting programs for digital computers have been written during the past five years for clever perception-transducing tricks. For example, Selfridge (1958) and Dineen (1955) demonstrated ways to sharpen edges and to find angles by means of local operations—that is, through examination of single spots in relation to their immediate neighborhoods only. Thus, noise is eliminated; the figure is made "good." Kirsch, Cahn, Ray, and Urban (1957) at the Bureau of Standards have programmed their computer to point to, delimit, compute the size of, and count separate objects. Here the computer is beginning to find figures in a ground. Uttley (1956) discusses perception by a conditional probability machine. This is a machine that does not merely follow rigid directions. Rather, it acts on the most likely alternative in an uncertain situation. Greanias, Hoppel, Kloomok, and Osborne (1957) at IBM have programmed their computer to recognize certain slope relations and line-segment sizes. Kamentsky (1959), at Bell Labs, working with a photoelectric mosaic reminiscent of the retina, can identify angles, endpoints, and closed loops. In all of these, a succession of one-step operations mirrors the layer-to-layer transformations in a logical or neural net. Unger (1958, 1959), by making use of combinations of these operations, has programmed an unusually successful computer.

A statistical approach leads to a rather different type of model (Clark and Farley, 1955; Taylor, 1959)—in its extreme form, Rosenblatt's Perceptron (1958a, b, c; 1959). In general, this is the class of machines that most consciously simulates the randomly connected, self-organizing aspect of the brain. Rosenblatt is extreme in that he posits connections that are completely ran-

dom in the sense that none of the original spatial ordering of the sensory input is preserved across the synaptic-like connections to the response mechanism. He claims to have proved that, by proper selection of certain parameter values, a machine can be built that will correctly classify and identify spatial (that is, dimensionalized, metricised) qualities and objects, despite the complete chopping up of the metric.

Relational processing methods

Finally, the analytical approach toward stronger relational analyses of forms. These methods, which seem to be the most promising and the most reasonable, are already giving indications of striking power. Sherman (1959) is writing a program for reading alphanumeric symbols by means of topological characterization, followed by identification of geometric properties such as lines and angles. Grimsdale, Sumner, Tunis and Kilburn (1959) have recently described a program they have written and run that has done all of these things and more, although in a less systematic way. They present examples of characters that their machine identified that are barely identifiable by a human observer. Their program is not in the least bothered by positional deviations or rotations. It identifies continuous segments, and characterizes lengths, slopes and curvatures. It builds up its own recognition tables. When it gets confused, it gives a precise probability-like estimate of its confusion. It was designed merely to identify printing. But, whereas the simpler commercially built machines will discriminate between a dozen or so standard figures, it has discriminated between a hundred or more classes of figures (that is, classes across the variations of type fonts).

Remember how completely almost any object is characterized by an outline drawing (that is, by its contour) and how thoroughly a contour can be analyzed into simple line segments, and how limited is the human's perceptual span. We can see how close this machine is coming to perceiving. Here, then, is a model for perception that really works. It suggests a simple set of basic elements into which any perceived object can be reduced, or mapped, so that transformations over typefaces and handwritings will all map into the same specification list.

Consider lines of eight possible lengths, eight possible slopes and eight possible curvatures. This is, roughly, the capacity of the human perceiver when he makes absolute judgments (Miller, 1956). It is probably larger than the number of meaningful discriminations needed to read printing. A program I am now coding will (I hope) follow line segments, while under the control of an assessing sub-routine, until it has identified a complete element as one of this 9-bits worth of possible elements (Uhr, 1959). It will identify the next element, store the relative location at which elements touch, and continue until it has completed the figure.

These elements seem almost "natural" ways of describing figures—especially man-made figures, such as letters. For example, an "A" equals a vertical left and a vertical right touching at the top; with a horizontal line joining their middles. A "B" is a vertical left with right top and bottom curved loops, both closed. An "R" differs from a "B" in that the bottom right line does not form a closed loop. Notice how imprecise the specification of a line's qualities can be, and probably should be. Even handwriting, when reduced to this sort of element, should give the standard characterization.

The letters to be "re-cognized" are stored in the computer's memory, along with their characterization lists—literally, what they look like. The computer first searches these lists to find out what to look for—what type of element to expect—in the figure it is trying to identify. Thus, the lists determine the directions in which the computer will search. They seem suggestive, in many ways, of our concept of "ideas" or "images."

Rather than using one standard list, the machine can also, in a pinch (that is, when it is uncertain, or when it gets a group of strange new objects), make use of its own processing powers to recharacterize and draw up a parallel list for the new unassimilable.

Before this processing begins, the figure is found in the field by means of a routine for sensing large homogeneous areas. Then

specific parameters are set—much like size- and object-constancy processing by humans—to be used for later normalizing and comparing of subelements. Figures are sharpened by a single process that does something much like drawing an average or essence-giving line through the figure. This process makes use of a Gestalt-theory concept: a force field toward closure and toward the "good" figure. Here, I would suggest (if it works), is another interesting indication that whole new sets of concepts are opened to operationalizing by the computer—and these not infrequently among those that have seemed especially fuzzy or intractable (indeed, possibly merely because of their complexity and power, which only now can be matched by an equally powerful methodology). A few aspects of Gestalt theory seem to fall naturally in place here—for example, the forces at play in illusions such as the Müller-Lyer, which seem, from this point of view, reminiscent of an essence line driving through a figure. This might account for the invention of that nemesis of the nonhuman machine, the (for people) harmless, almost helpful, serif.

There are, I think, some interesting machine-human comparisons, and cross-deductions, that can be tested on these machines that analyze relations. For examples: if, as seems to be the case, people take in about 20 bits of information per "glance" (10 to 200 milliseconds), will the machine be able to perceive as well, given this limit? If the machine is programmed to make only seven magical discriminations along the dimensions of form qualities, will the uncertainty and ambiguity thus programmed in mimic the human's uncertainty? Might this be a successful abstracting feature, in that it causes the machine to generalize and see "similarities" where people do? When the machine gets confused by superimposed figures, will they be the same figures that confuse people?

The way these programs make more complex upon more complex analyses is an intriguing thing about them. They examine relationships, make comparisons. They identify emergents, and finally wholes. They seem suggestive of wholistic and phenomenological approaches that we often tend to feel are too vague to operationalize.

REFERENCES

Bailey, G. E. C., & Norrie, G. O. Automatic reading of typed or printed characters. *Brit. Inst. Radio Eng: Conv. on Electronics in Automation*, 1957.

Broido, D. Recent work on reading machines for data processing. *Automation Progress*, 1958, 4, 183–185, 224–226.

Clark, W. A., & Farley, B. G. Generalization of pattern recognition in a self-organizing system. *Proc. West. Joint Comp. Conf.*, 1955, 86–91.

Denes, P. The design and operation of the mechanical speech recognizer at University College, London. *J. Brit. IRE*, 1959, 19, 219–229.

Dimond, T. L. Devices for reading handwritten characters. *Proc. East. Joint Comp. Conf.*, 1957, 232–237.

Dineen, G. Y. Programming pattern recognition. *Proc. West. Joint Comp. Conf.*, 1955, 94–100.

Dudley, H., & Balashek, S. Automatic recognition of phonetic patterns in speech. *J. acoust. Soc. Amer.*, 1958, 30, 721–732.

Farley, B. G., Frishkopf, L. S., Clark, W. A., & Gilmore, J. T. *Computer techniques for the study of patterns in the electroencephalogram.* Lincoln Laboratory Technical Report No. 337, 1957.

Forgie, J. W. A real time speech input for a digital computer. *J. acoust. Soc. Amer.*, 1958, 30, 668–669. (Abstract)

Friedberg, R. M. A learning machine: Part I. *IBM J. Res. Develpm.*, 1958, 1, 2–13.

Gelernter, H. Realization of a geometry theorem-proving machine. Paper presented at *Int. Conf. Information Proc.*, Paris, 1959. (Preprint)

Gelernter, H., & Rochester, N. Intelligent behavior in problem-solving machines. *IBM J. Res. Develpm.*, 1958, 2, 336–345.

Gilmore, H. F. The use of the Fourier transform in the analysis of visual phenomena. *Symposium on Pattern Recognition*, Ann Arbor, Michigan, 1958. (In press)

Glauberman, M. H. Character recognition for business machines. *Electronics*, 1956, 29, 132–136.

Gold, B. Machine recognition of hand-sent Morse code. *IRE Trans. Information Theor.*, IT-5, March, 1959, 17–24.

Greanias, B. C., Hoppel, C. J., Kloomok, M., & Osborne, J. S. The design of the logic for the recognition of printed characters by simulation. *IBM J. Res. Develpm.*, 1957, 1, 8–18.

Grimsdale, R. L., Sumner, F. H., Tunis, C. J., & Kilburn, T. A system for the automatic recognition of patterns. *Proc. IEE*, Part B, 1959, 106, 210–221.

Hagopian, R. H. Recognition of characters

printed in magnetic ink. *Automation Progr.*, 1958, 3, 332–334.

Heasly, C. C., Jr. Some communication aspects of character-sensing systems. *Proc. West. Joint Comp. Conf.*, 1959, 176–180.

Highleyman, W. H., & Kamentsky, L. A. A generalized scanner for pattern- and character-recognition studies. *Proc. West. Joint Comp. Conf.*, 1959, 291–294.

Hughes, C. H., & Halle, M. On the recognition of speech by machines. Paper presented at *Int. Conf. Information Proc.*, Paris, 1959. (Preprint)

Kamentsky, L. A. Pattern and character recognition systems—picture processing by nets of neuron-like elements. *Proc. West Joint Comp. Conf.*, 1959, 304–309.

Kazmierczak, H. The potential field as an aid for character recognition. Paper presented at *Int. Conf. Information Proc.*, Paris, 1959. (Preprint).

Kilburn, T., Grimsdale, R. L., & Sumner, F. H. Experiments in machine learning and thinking. Paper presented at *Int. Conf. Information Proc.*, Paris, 1959. (Preprint)

Kirsch, R. A., Cahn, L., Ray, C., & Urban, G. H. Experiments in processing pictorial information with a digital computer. *Proc. East. Joint Comp. Conf.*, 1957, 221–229.

Kovasznay, L. S. G., & Joseph, H. M. Image processing. *Proc. IRE*, 1955, 43, 560–570.

MacKay, D. M. Mentality in machines. *Proc. Aristot. Soc.*, Suppl. 26, 1952, 61–86.

MacKay, D. M. The epistemological problem for automata. In C. E. Shannon, & J. McCarthy, (Eds.), *Automata Studies*. Princeton: Princeton Univ. Press, 1956.

McCarthy, J. Programs with common sense. Paper presented at *Sympos. Mech. Thought Processes*, Teddington, England, 1958. (Preprint)

McCulloch, W. S. & Pitts, W. A logical calculus of the ideas immanent in nervous activity. *Bull. Math. Biophys.*, 1943, 5, 115–133.

Miller, G. A. The magical number seven, plus or minus two: some limits on our capacity for processing information. *Psychol. Rev.*, 1956, 63, 81–96.

Minsky, M. L. Artificial intelligence and heuristic programming. Paper presented at *Sympos. Mech. Thought Processes*, Teddington, England, 1958. (Preprint)

Newell, A., Shaw, J. C., & Simon, H. A. Report on a general problem-solving programme. Paper presented at *Int. Conf. Information Proc.*, Paris, 1959. (Preprint)

Rochester, H., Holland, J. H., Haibt, L. H., & Duda, W. L. Tests on a cell assembly theory of the action of the brain, using a large digital computer. *IRE Trans. Information Theor.*, IT-2, 1956.

Rosenblatt, F. *The perceptron. A theory of statistical separability in cognitive systems.* Buffalo: Cornell Aeronautical Laboratory, Inc., Report No. VG-1196-G-1, 1958. (a)

Rosenblatt, F. *Two theorems of statistical separability in the perceptron.* Buffalo: Cornell Aeronautical Laboratory, Inc., Report No. VG-1196-G-2, 1958. (b)

Rosenblatt, F. A probabilistic model for information storage and organization in the brain. *Psychol. Rev.*, 1958, 65, 386–407. (c)

Rosenblatt, F. Perceptual generalization over transformation groups. *Conf. Self-Organizing Systems*, Chicago, 1959, in press.

Samuel, A. L. Some studies in machine learning, using the game of checkers. *IBM J. Res. Develpm.*, 1959, 3, 210–229.

Selfridge, O. G. Pattern recognition and modern computers. *Proc. West. Joint Comp. Conf.*, 1955, 91–93.

Selfridge, O. G. Pandemonium: a paradigm for learning. Paper presented at the *Sympos. Mech. Thought Processes*, Teddington, England, 1958. (Preprint)

Shepard, D. H. & Heasly, C. C., Jr. Photoelectric reader feeds business machines. *Electronics*, 1955, 28, 134–138.

Shepard, R. N. Application of IPL: V to the simulation of perceptual learning. Paper presented at the *American Psychological Association*, Cincinnati, Sept., 1959.

Sherman, H. A quasi-topological method for the recognition of line patterns. Paper presented at *Int. Conf. Information Processing*, Paris, 1959. (Preprint)

Simon, H. A., & Newell, A. Heuristic problem solving: the next advance in operations research. *Operations Res.*, 1958, 6, 1–10.

Sprick, W. & Ganzhorn, K. An analog method for pattern recognition by following the boundary. Paper presented at *Int. Conf. Information Processing*, Paris, 1959. (Preprint)

Stevens, M. E. A machine model of recall. Paper presented at *Int. Conf. Information Processing*, Paris, 1959. (Preprint)

Steinbuch, K. Automatische Zeichener Kennung, *Nachrichtentechnische Zeit*, 1958, 11, 210–219, 237–244.

Taylor, W. K. Pattern recognition by means of automatic analogue apparatus. *Proc. IRE*, Part B, 1959, 106, 198–204.

Turing, A. M. On computable numbers, with an application to the Entscheidungsproblem. *Proc. London Math. Soc.*, 1936, 2–42, 230–265.

Uhr, L. Machine perception of printed and handwritten forms by means of procedures for assessing and recognizing Gestalts. Paper presented at *ACM Meeting*, Boston, 1959.

Unger, S. H. A computer oriented toward spatial problems. *Proc. IRE*, 1958, 46, 1744–1750.

Unger, S. H. Pattern recognition and detection. *Proc. IRE*, 1959, 47, 1737–1752.

Uttley, A. M. Temporal and spatial patterns in a conditional probability machine. In C. E. Shannon, & J. McCarthy, (Eds.), *Automata Studies*. Princeton: Princeton Univ. Press, 1956.

von Neumann, J. *The computer and the brain.* New Haven: Yale Univ. Press, 1958.

Theory Construction or Fact-Finding in a Computer Age?, *Charles Wrigley, Michigan State University*

My original intention when agreeing to participate in this symposium was to try to review the use of computers by psychologists both in retrospect and in prospect, discussing the two questions: How far have we come? And where are we heading?

There has been spectacular growth both in the number of university and research computer installations and in the number of psychologists using them. More than 100 universities have computers by now and the electronic computer is clearly going to replace the desk calculator and the punched card machine as our main computational aid. Time could usefully be spent considering the lessons of this development and the practical problems still to be resolved of organizing and staffing these computational centers.

But there seems to me to be a more important issue than these practical ones of hardware and personnel, and that is whether these technical developments are going to change our views upon psychological method —that is the problem to which I have decided to address myself today. Advances in instrumentation have often affected progress in other sciences. For example, chemistry benefited immeasurably from the development of glass instruments, astronomy from the telescope, biology from the miscroscope. The question I keep asking myself is: Will the computer do the same for psychology? In particular, will there be a change in the balance between theory construction and fact finding? Psychological methods have changed in the past. At one time theorizing of a philosophical nature was in high repute. So was the procedure of examining the contents and processes of one's consciousness. Later the study of overt behavior, and especially of animals, became focal, and the method of introspection began to look rather tarnished. After Freud, the method of free association gained a new respectability, and the couch became a standard piece of psychological equipment.

We live in the shadow of the Hullian theory construction, which has dominated the American psychological science for the past quarter of a century. Hull's methods were modeled upon those of Newton, as he understood them, and he undoubtedly aspired to be the Newton of psychology. The cornerstone of his psychological method was the hypothetico-deductive procedure. The first step was statement of a set of postulates and corollaries. In the final statement of his system, there were, as you will know, some 17 postulates and a further 17 corollaries. Consequences were deduced from these postulates and corollaries, and then experiments were designed to find whether the empirical observations agreed with the theoretically derived expectations, or hypotheses. If they did, the experiment was taken as supportive of the theoretical system. If they did not, then the postulates were revised and new conclusions deduced in the hope that the new set would better fit the experimental evidence.

The hypothetico-deductive approach has been very widely accepted. Theory and hypotheses have provided the start for the majority of investigations. Any study without this formal structure has been labeled "exploratory" and relegated to lower status. Each graduate student has had to go into the fray armed with his hypotheses, even though inevitably some of the hypotheses have partaken more of the form than the spirit of Hull. In a thesis I saw a few years ago, the author's hypothesis, presented boldly on the first page, was that his variables would prove to be associated, and thereafter, having given at least lip service to the prevailing ideology, he proceeded to a purely exploratory study in clinical psychology.

We now stand far enough away in time from the genius of Hull to see limitations as well as strengths in his approach. Theory is an ambiguous word in psychology. Sometimes we refer to what would be more precisely named "speculations," i.e., propositions supported only by very flimsy observational or experimental evidence, if any. These are the pre-scientific speculative theories of which psychology has already had ample measure in its scholastic days. Theories of personality have tended to remain of this type up to the present time.

Hypothetical constructs with their "surplus meaning" seem to me to have this speculative basis.

On the other hand, we sometimes refer to what might be better called "generalizations" which subsume under them an amount of detailed experimental evidence. Generalizations of wide scope may be dignified with the name of "law." The Weber-Fechner Law may perhaps serve as an example of experimentally-induced generalization. From this standpoint, intervening variables are to be regarded as constructs which summarize the experimental evidence. For example, "intelligence" is an intervening variable which cannot be observed but can only be inferred. When we judge someone to be intelligent, we are merely summarizing a number of observations and test situations in which he has performed effectively.

It seems easier to construct speculative theories than to test them experimentally. Many of them continue the pre-experimental scholastic tradition in psychology, and have no more scientific value than theories of alchemy and astrology. Even Hull, with his formal use of a geometric model, has overtones of scholasticism. Speculation seems to die hard in affairs of the human intellect. Repeatedly, scholars are more concerned with the coherence of ideas, one with the other, than with their correspondence with facts. But there are two ways in which speculative theories can be useful. The first relates to their imaginative and intellectual challenge. Occasionally a psychologist, by virtue of his talent for unexpected and unobvious theorizing, presents views which modify our outlook on human behavior despite their non-experimental character. I should classify the writings of Freud in this category. There still seems to be a place for myth-making in psychology, provided it is the myth-making of a genius.

The second use of speculative theories lies in their capacity to stimulate experimentation. A theoretical framework seems often to have a focusing effect in scientific endeavor, so that there is less likelihood of that rather aimless research which consists in counting or possibly merely recording in infinite detail the first observations which the investigator happens upon. Theorizing can play a problem-generating function. The theorizing is an amusing game which apparently helps the researcher in defining his problem and in devising a design for its investigation.

The example of Hull, however, warns us that we have to be very cautious in using theory in this way. The solid contributions to our science are the experimental findings, and not the theories which have been their precursors. If the experiment has been properly controlled and is replicable under those conditions, then the experiment becomes part of the enduring body of science, which no amount of subsequent theorizing can explain away. The theory stays, however, what it always has been, a purely speculative affair that will in nineteen cases out of twenty wither away, since it is highly unlikely in these early days of psychology's history that we should be able to arrive at generalizations which will indeed fit a very wide range of phenomena. It seems to be that this is illustrated by Hull's own history. The experiments which he inspired are his main scientific contributions. In retrospect his theories are coming to appear more and more contrived, and less and less useful in predicting the behavior of a rat, let alone that of a man.

Theory construction dangers

The danger of a theory is obvious. The theoretician can easily become more devoted to the theory than to the experimental findings. When using the hypothetico-deductive method, failure of the experimental findings to agree with the theoretical predictions is supposed to indicate needed revision of the theory. But when results do not turn out as hypothesized, the investigator is in a dilemma. All too often, he clings resolutely to his theory, and prefers to try in some fashion to explain away his or his rival's experimental evidence. Nor is the position much happier for him if he follows the alternative policy of accepting the experimental verdicts and then constantly modifying this theoretical structure. After a while, his so-called theory changes into a quagmire of uncertainty and doubt.

These difficulties are illustrated in the

history of learning theory. Hullians and Tolmanians had recourse to the same body of experimental findings; yet their theories did not merge. It proved very difficult to design critical experiments which would decisively decide between the rival interpretations. It always seemed possible to explain the Hullian results from the standpoint of the theories of Tolman and vice versa.

In accepting Newtonian physics as his model, Hull seems to me to have erred in two ways. The first is that classical physics had been far more inductive and fact-finding and far less theoretically oriented in its growth than he supposed. It was not that Newton started with theories of planetary motion and then, deducing the consequences, confirmed by astronomical observations that this was how the planets actually moved. It was rather that the painstaking observations had been made by Tycho Brahé nearly one hundred years before, and it was these observations that Kepler and then Newton were stimulated to try to account for. A similar priority of fact-finding occurred incidentally in biology. It was not that Darwin formalized the evolutionary theory and then sought evidence in its support. Naturalists had collected living and fossil evidence for centuries. Darwin himself had important field experience. It was only after thorough perusal of the facts that he proceeded to construction of his theory.

Hull's second error lay in his selection of physics as the model science for psychologists to emulate. The movements of a planet seem to be determined by far fewer antecedents or stimuli than the behavior of a child. If an analogy must be taken from the physical science, then we should turn to meteorology rather than to astronomical physics. Far more antecedents have to be taken into account in deciding when it will next rain than when the next eclipse will be. It is not surprising that the Weather Bureau has become a major computer user, and that the larger the computer memory, the better.

Hull hoped for a psychological theory which would arise, like Ariadne, fully-fashioned from the waves. I agree entirely with Broadbent (1958) when he points out that Hull's expectations were so specific that it was hardly likely that they would be fulfilled, and that it would have been better tactics to have asked more general questions which would indicate the theoretical direction in which the answer might lie. As Broadbent puts it, "The ideal at early stages is to perform experiments with only two possible results, and to say, 'If A occurs, a theory of this general type must be true; if B occurs, it cannot be.' "

But even Broadbent, so it seems to me, is too concerned to gird himself with some theory. Why, we may ask, has psychology not attained the high degree of precision in its laws that physics has? The plea is often made that this is because psychology is such a young science. With respect to the experimental treatment of psychological data, this is true. It is still less than one hundred years since Fechner published his pioneer work. But in another way our subject is really a very old one. Judging from the early manuscripts, humans seem to have been pondering about behavioral complexities for just as long as they have about the movements of the heavenly bodies. The fundamental difference, I suspect, is the greater complexity of psychology. Prediction is always hazardous, especially when it is a negative prediction, but my guess is that we shall never attain to the rigorous and tightly-structured theories of physics. As I see it, our task in psychology is to establish functional relations between predictors (or antecedents) and predictands (or consequents). These functions are likely to be probabilistic in nature, and to contain multiple predictors and multiple predictands. The more predictors that are included in our equations, the higher will be the probability with which we can predict the outcome. These functional relations will be the generalizations which will correspond with the theoretical laws of physics. Let me examine more closely the situation in which the weather forecaster finds himself. He, too, wants to establish functional relations between a set of predictors (viz., present temperature, humidity, pressure, air movement, etc., as reported by a network of stations) and a set of predictands (temperature, precipitation, etc., as they will be in Cin-

cinnati tomorrow). Now the weather, as you will know, is determined in large measure by atmospheric movements, and these movements are determined in turn by physical laws governing pressure and temperature of the gases which comprise the atmosphere. These laws are mostly known and of a high degree of precision. Meterology thus has a dependable theoretical basis in physical law to a much greater extent than psychology has in biological law. Yet, despite the availability of these well understood and generally accepted physical laws, the school of weather forecasters at Massachusetts Institute of Technology is finding that it can make just about as accurate forecasts by excluding these laws from the analysis as by including them. That is to say, forecasting can proceed in either of two ways. Rational deductions can be made from the present state of the weather system, in terms of the known physical laws, as to what the weather will be like tomorrow; or alternatively, past observations and their outcomes can be utilized on a purely probabilistic and statistical basis to determine appropriate weights for predicting outcomes for new weather configurations probably never before recorded. Present evidence is that the second, which excludes theory, is just about as effective as the first, which includes it.

Likewise in psychology, what we want to do is to establish functions between predictors and predictands. Given a certain stimulus configuration and a certain organismic heredity, experience, and contemporaneous state, how will the organism behave? Why not try to establish the functions between stimuli and responses directly and without reference to any preconceived theory? On this view, even intervening variables are not to be regarded as necessities but only as matters of operational convenience. The intervening variable, as adopted by Tolman, seems to have been introduced to account for intra-organismic and inter-organismic variability in responding to the same external pattern of psychological stimulation. Differences in overt behavior were to be attributed to inferred differences in reaction potential, habit-strength, drive, intelligence, etc. Now reaction potential has to be inferred from latency and reaction amplitude. Why then include reaction potential in our function? We might do better to restrict the function to the measurable latency and reaction amplitude.

The position developed here might seem to imply a concern only with the input and the output of the organism, and a disregard for its neurophysiology. That is not so. A function linking input and output is useful but is only part of the story. It would add to our factual knowledge to determine a further function linking behavior to neurophysiological change. This latter function provides us with better knowledge of the intermediaries which link the input and output of the organism.

The traditional scientific method has required only one predictor to be changed, and the effect determined upon a single predictand. This simple procedure was needed since there was no statistical or computational way of disentangling the effect of a number of predictors operating simultaneously. Theory was then helpful in providing a rational criterion for selecting one rather than another predictor for experimental manipulation. But multivariate statistics is making it possible to disentangle the effects of a number of predictors varying simultaneously, and the work, because of the development of computers, is no longer intolerably lengthy. The main influence of computers, I want to suggest to you, will be that the search for functional relations can be carried out directly rather than by the mediation of theory.

Let me finish by briefly summarizing my argument. Facts are more enduring in science than theories. The theories change in terms of new observations. The facts, provided they are properly collected and are replicable, become part of the permanent body of scientific knowledge. Computers are making possible a much more intensive search for multivariate functional relationships. Theory therefore becomes less important in guiding us as to what to focus on. Some psychologists should theorize, but it is by no means essential to do so in the acquisition of new psychological knowledge.

REFERENCE

Broadbent, D. E. *Perception and communication.* New York: Pergamon Press, 1958.

Discussion, *Philip A. Smith, System Development Corporation*

Recently, someone commented that as each psychologist learns about computers, he has new ideas about ways in which they can be applied. This point was well-illustrated by our speakers here this afternoon. They gave a broad sample—but it is significant that it was only a sample—of some recent and projected uses of computers in psychological research. As our chairman anticipated, it was hard to hold them strictly to a recounting of past accomplishments. They were not content to dwell very long on things already done, but as computer enthusiasts are prone to do, described tasks they are about to undertake and told of possibilities opened by new computer developments. There was one thing we did not hear much about: the common pitfalls, shortcomings, and dangers in utilizing computers when undertaking research—such things as experimenter carelessness in the selection of variables for study (since large numbers of variables are so easily handled), or the tendency to fit the data to whatever statistical routines are available, in order to avoid the inconvenience and delay of writing new programs—and so on. There are many others.

In the discussion of his preliminary work on automated teaching, Dr. Silberman told of using people to simulate certain computer functions. This is a reversal of what we typically hear, and appears unique, although some will claim that graduate students have been used in this capacity for many years. Seriously, I think that Dr. Silberman has undertaken work in a vital and formidable area. He implied that the use of a computer in a teaching situation might not be limited to pre-programmed sets of data, but could demand computer generation of new sets of items tailored to the learner. This seems to call for creativity and reasoning ability on the part of the computer as well as the experimenter.

We have had a little fence-straddling on the issue of computer approximation of human judgment. For example, one portion of Dr. Holtzman's excellent paper was a bit like a clinical psychologist's report—it could be interpreted several ways. I would like to know how firmly astride the fence he and also Dr. Vandenberg are, and whether they lean or may be pulled to one side or the other on this question of computer preemption of the diagnostician's intuitive processes. Dr. Vandenberg also touched on some legal, ethical, and attitudinal variables that may be involved if computers are utilized in this manner.

I believe Dr. Uhr's comment on partially random, self-organizing machines, of which an early example may be Rosenblatt's perceptron, is enormously challenging and stimulating. In this category also are Grimsdale's and Uhr's own computer perception programs. I was privileged to read an extended version of Uhr's paper, in which he touched on the development of consciousness and awareness in machines. He was convincing, and I was inclined to believe that with Dr. Uhr as its tutor, a computer might develop an awareness of self, or at least learn to lie convincingly when questioned on its state of consciousness. I am sure there will be some questions from the audience on this point.

Mention has been made by several of our speakers of Newell, Shaw and Simon's work on heuristic programs, which show good promise of playing championship chess, composing musical scores, discovering proofs to logic theorems—in short, creative thinking and reasoning. Such programs do not operate by brute trial and error of all possible alternatives in solving a problem, but mimic the human problem solver, providing good and poor solutions and sometimes no solution at all. One thing seems sure—whether it be clinical judgments, medical or psychiatric diagnosis, or effective teaching—we are likely soon to be forced into the possibly embarrassing position of having to open our thinking processes to inspection and to test.

One final comment, this on Professor Wrigley's delightful and scholarly paper. In his brief for fact-finding as opposed to theory-building, Dr. Wrigley quietly put to bed Tolman, Hull, and Freud. If I may mix a metaphor, I'm not so sure our other speakers or our audience will be content to let these sleeping dogs lie.

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

FIELD, M. J. **Mental disorder in rural Ghana.** *J. Ment. Sci.*, 1958, 104, 1043–1051.

This anthropologist-psychiatrist studied mental illness in the villages of rural N. W. Ghana, where only dangerous psychotic patients are referred to mental hospitals. Such mental illness finds expression at the native shrines where sick and well supplicate for "protection," in return for which the pilgrim makes vows. Sometimes the breaking of vows leads to acute transient psychoses of "fear and guilt frenzies." Often patients have to be brought in fetters to plead for forgiveness. Follow-up studies showed that many of the patients who had really transgressed had an underlying schizophrenic process, whereas depressive patients often accused themselves of witchcraft. The author felt that attempted use of "bad medicine" represented a schizoid handling of aggression instead of the usual native quarrelsomeness. He compares this to the use of the Black Mass by schizophrenics in the Middle Ages.

About 1% of a surveyed population of 4,200 was found to be schizophrenic. It was more apparent in the literate segment of the population, probably because in the easy-going draught-playing illiterate group the illness could pass unnoticed. Field predicts an increase in manifest schizophrenia with implementation of new government policies of universal literacy, industrialization, and compulsory non-military training camps for unemployed literates.

Field postulates that some kinds of magic ritual, totemism, and witchcraft were originated historically by original schizophrenic minds and later adopted by less creative normal minds.

The author points out the widespread use of stereotyped dreams, e.g., an angry deity chasing one; and everyday metaphor, e.g., use of a snake as an evil person rather than a phallic symbol.

Hysterical dissociation, which formerly was utilized only by women who served as mediums and mouthpieces of the gods in native ceremonies, has been incorporated now by certain Christian cults, whose adherents are possessed by the Holy Ghost, then attain a peaceful euphoria and function as good, generous, hardworking, happy citizens. (*JD*)

CLARK, D. F. AND ACKOFF, R. L. **A report on some organizational experiments.** *Operations Research*, 1959, 7, 279–293.

The Operations Research Group at the Case Institute of Technology has been conducting exploratory studies in the area of organizational structure for the past five years. This report presents the results of some preliminary research on the effect of the organization structure on efficiency of performance. Although the long-term objective of the study is to develop a mathematical theory of organization, the authors claim that their project at its present stage offers only a methodological interest and is intended to be merely suggestive for further research.

The study is based on the use of an operational game involving a small-scale organization in a laboratory situation. Variations in the composition of the participating teams or in the rules of the game are considered analogous to changes in the structure of a real organization and in the problems confronting it. The operational game is a word game, consisting of three basic operations: buying letters, making words from them, and playing the words on a board. These operations are performed, in that order, by the "Purchaser," the "Manufacturer," and the "Salesman." In some cases the team has a fourth member, the "Executive," who is

responsible for coordinating team effort and directing strategy. The performance of each operation involves an expense, and each word played on the board yields an income. The goal of the group is to maximize profit in a given period of time.

A conflict of interests has been built into the game. If the Purchaser tries to minimize his expenses by buying cheaper letters (the less frequent in the English language), the Manufacturer will have more trouble in making words. Also, the interests of the Salesman, who is supposed to place the words on the board according to special rules, will often conflict with the Purchaser's and Manufacturer's tendency to keep their expenses low. Compromises between the players are therefore needed for effective group performance. One of the functions of the Executive is to determine what kind of compromise will be both acceptable to the players and efficient for group performance. Various degrees of complexity in the organization can be developed by combining teams according to different principles of management.

A number of tentative conclusions were reached: (1) Teams with an "Executive" do better than the teams without an "Executive." (2) Absence of communication between the players (in the non-Executive teams) tends to lower the team scores. (3) Team performance is not related to individual skills. (4) The introduction of various degrees of conflict between members of the team affects performance. "Cooperative" teams have a larger word output, but their costs are relatively high; in "conflict" teams there is an indication that results are in the opposite direction. (*RT*)

CHOMSKY, N. **Review of *Verbal Behavior* by B. F. Skinner (Appleton-Century Croft, 1957).** *Language*, 1959, 35, 26–58.

Skinner's "functional analysis" tries to provide a way to predict and control verbal behavior by observing and manipulating the physical environment of the speaker. The terms *stimulus*, *response*, and *reinforcement*, derived from the animal laboratory, are extended in such a way that "if we take them in their literal meaning, the descriptions cover almost no aspect of verbal behavior, and if we take them metaphorically, the descriptions offer no improvement over various traditional formulations."

Take first, *stimulus*. For Skinner, if we look at a red chair and say "red," the response is under the control of "redness"; if we say "chair," it is under the control of a collection of properties, chairness. Since properties are free for the asking, the word "stimulus" has lost all objectivity. Stimuli are no longer part of the outside physical world; they are driven back into the organism. The controlling stimulus need not even impinge on the responding organism, as when a traveller recounts events seen abroad. We identify a controlling stimulus when we hear the response. We cannot control the property of a physical object to which an individual will respond, except in highly artificial cases.

Consider *response*. In Skinner's animal experiments, the operant is defined by fiat, e.g., a peck or a bar press. The operant is thus defined with respect to a particular experimental procedure. The verbal operant is defined as a class of responses of identifiable form functionally related to one or more controlling variables. No method is suggested for determining in a particular instance what are the controlling variables, how many such units have occurred, or where their boundaries are in the total response. We are left with no way of justifying one or another decision about the units in the verbal repertoire.

Consider *reinforcement*, which Skinner considers a necessary condition for language learning. Much of this class, however, is made up of automatic self-reinforcement (as when one talks to oneself) and verbal activities where the behavior constitutes its own reinforcement. It seems that Skinner's claim that all verbal behavior is acquired and maintained in strength through reinforcement is quite empty, because his notion of reinforcement has no clear content, functioning only as a cover term for any factor, detectable or not, related to acquisition and maintenance of verbal behavior.

The terms borrowed from experimental psychology simply lose their objective meaning with this extension, and take over the

full vagueness of ordinary language. The elimination of the independent contribution of the speaker and learner (a result which Skinner considers of great importance) can be achieved only at the cost of eliminating all significance from the descriptive system, which then operates at a level so gross and crude that no answers are suggested to the most elementary questions.

We need a prior understanding of what is acquired before we can speculate about the process of acquisition. Study of the actual observed ability of a speaker to distinguish sentences from non-sentences, detect ambiguities, etc., apparently forces us to the conclusion that his grammar, a finite device to enumerate an infinity of new sentences, is of an extremely complex and abstract character. And the young child has succeeded in carrying out what from the formal point of view seems to be a remarkable type of theory construction. If this is correct, we can predict that a direct attempt to account for the actual behavior of speaker, listener, and learner, not based on a prior understanding of the structure of grammars, will achieve very limited success. The grammar must be regarded as a component of behavior which can only be inferred from the resulting physical acts. The fact that all normal children acquire essentially comparable grammars of great complexity with remarkable rapidity, and without carefully arranged training, suggests that human beings are somehow specially designed to do this, with data-handling or hypothesis-formulating ability of unknown character and complexity. (*WPL*)

UTTAL, W. R. **A comparison of neural and psychophysical responses in the somesthetic system.** *J. comp. physiol. Psychol.*, 1959, 52, 485–490.

An experimental technique for comparing the subjective responses corresponding to observed neural signals is described in this paper. Electrical impulses are delivered to the ulnar nerve of subjects by skin electrodes placed over the wrist. The nerve response is measured by a recording electrode placed on the skin above the elbow and these records are compared with estimates of stimulus magnitude made by the subject.

A double pulse method of stimulation was employed. It was found that two stimuli could not be resolved as separate pulses if the inter-pulse interval was less than ten milliseconds. As the inter-pulse separation was reduced to values below this, the amplitude of the second response pulse eventually decreases as the second stimulus moved into the refractory period of the first stimulus. The range of inter-pulse interval was varied from 0.8 milliseconds, where the second response had vanished, to 3.7 milliseconds by which point the second response had the same magnitude as the first response with no further increase for longer intervals.

The psychophysical estimates of magnitude were based on a scale with magnitude 10 assigned to the sensation obtained from a reference stimulus using a pulse separation of 3.7 milliseconds. Only the inter-pulse interval was varied in the course of the experiment; all other variables were kept constant.

The compound action potential recorded from the arm was a sharp spike showing no evidence of any spread due to different velocities in the response. Furthermore, the latency of the second response did not vary as the inter-pulse interval decreased indicating that the inter-pulse interval had no observable effect on either the conduction velocity or the time required to initate a response.

In the region where the amplitude of the second pulse was dependent on the inter-pulse interval, the relationship was approximately linear. The estimate of magnitude showed a similar variation. The results of these measurements lead to the conclusion that the subjective experience is not simply coded by the interval between successive responses, but, within the time interval studied, by the sum of the response amplitudes. (*WJH*)

BENDA, C. E. AND MELCHIOR, J. C. **Childhood schizophrenia, childhood autism, and Heller's disease.** *International Record of Medicine*, 1959, 172, 137–154.

The authors use both psychiatric and organic bases to construct a working differentiation of childhood schizophrenia, childhood autism, and Heller's disease. They

do not integrate the concept of symbiotic schizophrenia into their system.

The term "childhood autism" is used as Leo Kanner defined it, meaning self-sufficient, without interacting with others. They feel it does not necessarily presuppose a schizophrenia of childhood, since some patients improve to normal functioning at puberty. Etiologic agents include asphyxia at birth, metabolic disorder, severe early emotional deprivation, or childhood schizophrenia.

Their classification of childhood schizophrenia includes groups whose illness originates (1) in puberty and pre-puberty, (2) in the 6- to 10-year group, and (3) in earliest phases of development. The diagnosis is relatively easy in the former two groups because of personality changes, secondary withdrawal, extreme compulsive patterns, paranoid ideas, and hallucinatory experiences. In the group that never appeared normal, however, the differential diagnosis is more difficult, since these children may resemble autistic children or children with Heller's disease. They have noted some cases in which the child improves between the ages of six and fourteen but then reverts to a more classic schizophrenic appearance at adolescence. They question the use of poor ego boundaries as sufficient basis for the diagnosis of schizophrenia. Childhood schizophrenics may be abnormally bright and function so, whereas an autistic child may give evidence of being bright but untestable.

Heller described in 1930 a disorder including mental retardation becoming apparent in the third and fourth years, moodiness and screaming irritability, regression in cleanliness, loss or deterioration of language, and retention of a clear intelligent look. The theories presently held are that (1) Heller's disease is a clinical syndrome without a single etiologic agent; (2) it is a post-encephalitic condition; (3) it is related to childhood schizophrenia; (4) it is a progressive deteriorating metabolic disorder. Benda and Melchior describe an autopsied case with small cerebellar and cerebral granulomas, vasculitis, and unusual multinucleated giant cells, possibly caused by *Histoplasma capsulatum*. They suggest that cases of this type be studied for mycotic infections since these are treatable and potentially curable. (*JD*)

Let me here remind you that the essence of dramatic tragedy is not unhappiness. It resides in the solemnity of the remorseless working of things. This inevitableness of Destiny can only be illustrated in terms of human life by incidents which in fact involve unhappiness. For it is only by them that the futility of escape can be made evident in the drama. This remorseless inevitableness is what pervades scientific thought. The laws of physics are the decrees of Fate.

A. N. WHITEHEAD, *Science and the Modern World*

ABOUT THE AUTHORS

John C. Harsanyi ("Explanation and Comparative Dynamics in Social Science") is senior fellow in the Research School of Social Sciences at the Australian National University, Canberra, Australia. He is a graduate of Budapest University (Hungary), Sydney University (Australia), and Stanford University. By education a mathematical economist, he is also interested in philosophy of science and the methodology of the social sciences. His main publications are on utility theory, welfare economics, ethics, and game theory, and have appeared in economic journals such as the *Journal of Political Economy*, *Review of Economic Studies*, and *Econometrica*, as well as in the philosophical journal, *Mind*. He also wrote a chapter in the Princeton *Contributions to the Theory of Games*, Volume IV. He writes: "My present paper grew out of my interest in the problem of explaining economic and social development, and in particular in explaining the divergences in the development of different human societies. I have always felt that in a sense this is really *the* main theoretical problem of the social sciences. But it is a very difficult problem, and so far, we seem to have made rather little real headway with it. I think, however, that the modern concept of analytical models now has given us a theoretical tool with great potentialities for explaining social development." At present, Dr. Harsanyi is doing research in game-theoretical bargaining models for explaining social equilibrium and the balance of power between different social groups.

Wayne H. Holtzman (Abstract of "Can the Computer Supplant the Clinician?") is professor of psychology at the University of Texas and associate director of the Hogg Foundation for Mental Health. His research interests are concerned with personality measurement, projective techniques, research design and methodology in the behavioral sciences.

Charles E. Osgood ("The Cross-Cultural Generality of Visual-Verbal Synesthetic Tendencies") is director of the Institute of Communications Research and professor of psychology at the University of Illinois. He took his undergraduate training at Dartmouth and received his Ph.D. from Yale University. His main interests are in the study of language behavior, particularly psycholinguistics and the nature and measurement of meaning. The topic of synesthesia has been a focal interest since his undergraduate days at Dartmouth, where his B.A. thesis combined experimental studies of auditory-visual-verbal synesthesia with cross-cultural comparisons of metaphors. Concerning this paper, he writes: "The Southwest Project in Comparative Psycholinguistics (SWPCP for short) brought together teams of linguists, anthropologists, and psychologists to study relations between language and cognition on a variety of levels. It offered an ideal opportunity to explore the generality of synesthetic tendencies. I would like to think that the technique described in this article, which is basically very simple and easy to administer, will be applied by other investigators to other cultures, as opportunities arise. It was a pleasure to discover that some aspects of human cognition, at least, are contra-Whorfian—general to the human mind. One of my students, Mr. Murray Miron, is presently working on a cross-cultural study which we expect will show that certain onomatopoeic tendencies (connotative sound symbolism) are also quite general. Perhaps we shall find that, even though the denotative rules of usage in language are highly arbitrary, the connotative "guts" of meaning systems are widely shared."

Arthur K. Shapiro ("A Contribution to a History of the Placebo Effect") did his undergraduate work at City College of New York and received his M.D. from the University of Chicago Medical School. After his psychiatric residency at the Massachusetts Mental Health Center, Boston Psychopathic Hospital, Harvard Medical School, and the Albert Einstein College of Medicine, he joined the staff at New York University—Bellevue Medical Center as a research psychiatrist. He reports that "During the course of a research project on CSF (cerebral spinal fluid) and schizophrenia, I had an opportunity to extensively review the psychiatric literature from 1920 to the present time. I was struck with the 'claim upon claim' for various curative methods and the dramatic pathological findings in psychiatric patients—the vast majority of which have never been heard of again and have since gone into the oblivion of psychiatric history, but without having been adequately retired or recounted from the literature. This was the beginning of my interest in the placebo effect, and I found myself attracted to, reading about, and then collecting bibliographic material on methodology and the placebo effect. This bibliography increased as I undertook

other research: CSF, serotonin in schizophrenia and other conditions; transaminase and protein electrophoretic studies in schizophrenia; primitive sensory stimuli and polygraphic responses in schizophrenia; etc." Dr. Shapiro is continuing research on the placebo effect and has other articles on this research in press and in preparation.

Harry F. Silberman ("A Computer as an Experimental Laboratory Machine for Research on Automated Teaching Procedures") is a graduate of U. C. L. A. where he received his Ed.D. degree in educational psychology. He is presently human factors scientist at the System Development Corporation at Santa Monica, California. He reports that he is generally interested in the application of data processing methods to educational problems, is currently conducting research on automation, and working on an experimental computerized teaching machine.

Philip A. Smith ("Discussion" in Symposium on The Impact of Computers on Psychological Research), a psychologist at the System Development Corporation at Santa Monica, California, did his undergraduate work at Miami University and received his graduate degrees from the University of Kentucky. He writes, "My present interest in the growing range of digital computer applications was originally stimulated by Professor E. Lowell Kelly. Several years ago, when I was working as a clinical research psychologist at the Ann Arbor VA Hospital, he suggested that I use the University of Michigan's IBM 650 computer to process some psychiatric research data I had gathered. This led inevitably to larger research efforts, to the need for a larger computer to analyze the data, and also some awareness of computer developments in other areas. At the System Development Corporation, my interest soon broadened to very large, real-time data processing and control systems. At SDC I have been involved in the development of training programs for the men in these systems."

Leonard Uhr ("Intelligence in Computers: The Psychology of Perception in People and in Machines") is associate research psychologist at the Mental Health Research Institute of The University of Michigan. A previous contributor to the Computers in Behavioral Science department of *Behavioral Science* (July, 1959), he is the author of numerous articles on the behavioral effects of psychoactive drugs and is co-editor, with James G. Miller, of a forthcoming book, *Drugs and Behavior*, to be published by John Wiley and Sons, Inc. He received his B.A. from Princeton, an M.A. (in writing) from Johns Hopkins, and a Ph.D. from The University of Michigan in psychology.

Steven G. Vandenberg ("Medical Diagnosis by Computer: Recent Attempts and Outlook for the Future"), associate research psychologist at the Mental Health Research Institute, The University of Michigan, is editorial director of the Computers in Behavioral Science department of *Behavioral Science*. An associate director of a schizophrenia and psychopharmacology research project, he is interested in the development of psychological tests for schizophrenia, and in data analysis. Dr. Vandenberg, the author of numerous articles, did his undergraduate work in The Netherlands and received his Ph.D. in psychology from The University of Michigan.

Charles F. Wrigley ("Theory Construction or Fact-Finding in a Computer Age?"), professor of psychology at Michigan State University, received his M.A. from the University of Auckland (New Zealand) and his Ph.D. degree from the University of London. He is the author of various papers relating to: (1) application of multivariate statistics in psychology, especially factor analysis, multiple correlation, and canonical analysis; (2) use of electronic computers by psychologists; (3) scientific method in psychology, especially probability functions and the place of theory in science; and (4) political psychology, particularly legislative voting patterns and liberal-conservative attitudes.

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Think of the great advantage to political parties and political candidates if there were some reliable way of knowing the best strategy to follow for winning an election. If such candidates as Senator Kennedy or Vice-President Nixon could assess the competitive structure of a state, they would be able to define one major aspect of the opportunity structure of that state. Here is an interesting attempt to develop a measure of competitiveness in party relationships—an unusual approach to a problem in political science and practical politics.

THE STRUCTURE OF COMPETITION FOR OFFICE IN THE AMERICAN STATES*

by Joseph A. Schlesinger

Michigan State University

THE critical variable in the study of American politics in the states and localities has been the level or degree of inter-party competition. Such factors as party cohesion and organization (MacRae, 1952; Key, 1949), contests for party nominations (Turner, 1953), the structure of political careers (Schlesinger, 1957, pp. 97–102), social and economic conditions (Golembiewski, 1958; Key, 1956, Ch. 8), and the quality of government (Lockard, 1959, Ch. 12) have all been related to party competition. The political analyst has found, within the all-embracing national competitive two-party system, that the aberrations from the national system are a useful variable in studying political behavior. Considering the importance of the competitive variable to such studies, remarkably little has been done to clarify the concept and create measures of comparable utility. In the following we shall examine some of the difficulties involved, and develop a typology of competitive structures which will take into account several factors in their measurement.

The main reason for the failure to investigate the concept of competition is that its meaning has always been clear within what may be called the "literary" or "classical" theory of the two-party system. This approach, which starts from the assumption of only two parties vying for control of government, has one essential assumption: that one and only one party can be called the government, while the other is necessarily the opposition.[1] As long as each party has a realistic expectation that it may take over the government, then there is competition. From such a small set of assumptions it is possible to elaborate quite a complex and attractive political system, explaining much of what we know today about policy making and voter behavior. The finest demonstration of the classical party theory is in the recent work of Anthony Downs (1957), who has combined elements of the widely current approach in political science with the more rigorous methods and assumptions of economic theory. The result is extremely useful, and not least of its uses is that it lays down clearly what are the assumptions about political behavior upon which our thinking has been based.

Whereas much of the criticism directed at

* This study is part of a general analysis of leadership recruitment in the American states, which has enjoyed the support of the Social Science Research Council and its Committee on Political Behavior.

[1] The best statement of the two-party system as a model toward which political reform should be directed is in Schattschneider (1942) and the report of the Committee on Political Parties of the American Political Science Association (1950). These studies have been less concerned with description of how the system works than in devising ways of making it work like the model.

Downs' book takes exception to his axioms about the "rationality" of voters and parties, no critic, to my knowledge, has pointed out the lack of realism for the United States in his basic assumption that there is a government and an opposition party clearly marked as such (Diamond, 1959; Rogers, 1959). Yet as Key and Silverman (1954) have demonstrated, divided control by the parties of state legislatures and governorships is very common. And the control of the presidency by one party and one or both houses of congress by the other has been as frequent since 1946 as the presumed "normal" condition of unified party control, and may in the future even become the more usual condition. In such circumstances, it is impossible to say that one party is the government and the other the opposition. This major departure from the assumptions of classical party theory has relevance not only for questions of party strategy and public policy, but it also muddles the very notion of competition itself. For once the logical neatness of the government-opposition arrangement breaks down, the concept of party competition has to be related not to the whole, but to the parts, and then put together again. It is, of course, the constitutional arrangements of both separation of powers and federalism which permit electorates to react differently to the various offices. And not only may electorates distinguish in their choices among the federal, state, and local levels, but within each category they are usually permitted to choose a variety of officers mixing the parties, and further confounding the government-opposition framework. That in fact electorates do behave in this way is evident to any casual observer of state elections. Therefore an accurate reflection of the degree of inter-party competition in a state ought to allow for such divergence from office to office.

A measure of competition must take into account, then, not only the closeness of the parties to each other in their ability to capture office, but also the variations within a state political system in that closeness. For that reason our measure of competition will be based initially upon the degree of competition for individual offices, and from these measures will be constructed the total competitive framework for a state. For the problem of measuring competition for a single office is the same as that in the classic government-opposition system, and that concept is straightforward.

But if the concept of competition in a simple two-party system is unambiguous, its measurement is not. For one thing, it is necessary to rely upon past data to determine the state of competition, and this immediately poses the problem of how much past electoral history is worth looking at. Here the disposition of the observer and the uses he wishes to make of the measure are the determining factors. For the measure, however derived, will be but a description of the period examined, and therefore ought to cover the same period as the other variables to which competition will be related. In the scheme to be presented we shall use the electoral history of the states from 1914 to 1958. There are two reasons for this particular period. One, it encompasses the period since popular election of United States senators was made mandatory in all of the states. And secondly, it covers most of the time span of a companion study of the careers of state officials. The measures derived, however, should be of utility for any analysis of political phenomena since 1914.[2]

MEASURES OF COMPETITION

Once the time period is established the actual measure of competition must still be selected. There are three measures of competition which have been used in the literature: during any time period we may examine (a) the extent of division of control of office between the parties; (b) the rate of alternation of control of office between the parties;[3] and (c) the relative closeness of the

[2] It should be clear that by measuring competition in a state in the past, one is not necessarily making predictions, except in the sense that any accurate description of past behavior would lead one to expect, barring any changes in the conditions of that behavior, that it will continue in the future.

[3] These two measures are examined in Schlesinger (1955). The present technique is an adaptation of the one used there to examine competition for just the office of governor.

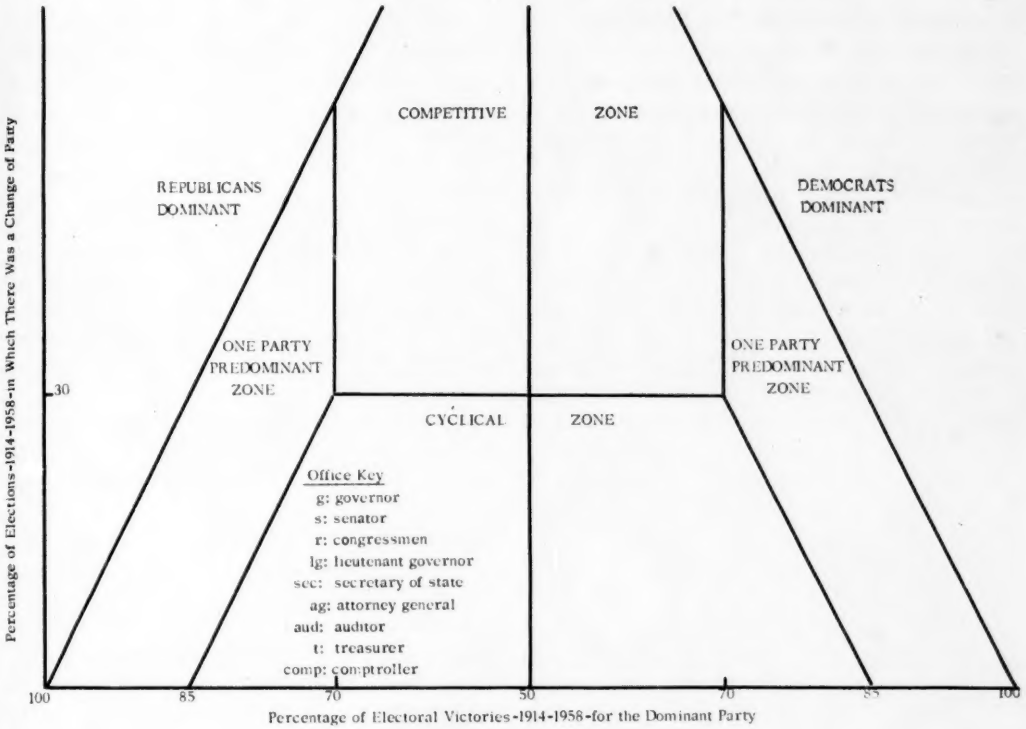


FIG. 1

vote.[4] All three measures are related, but it is possible, within the mechanics of the system, to get three quite different results from each measure. For example, there may be an electorate which is very closely divided but so stable that there is never an alternation of parties. Or a very volatile electorate may swing from one party to the next and provide large margins of victory. And then again, a state may over the total period show a close division of party control of office, but the measure may mask a situation in which each party has controlled the office for very long cycles.

The scheme we shall use combines the first two measures on a scatter diagram.[5] (See Figure 1.) The exclusion of the third measure, the closeness of the vote, means that voting behavior which has not had any impact upon office holding will be ignored. As a result, the measure loses some utility as a predictive device, since it will not reveal any trends towards or away from a competitive situation. Thus for instance, in Vermont, with the sole exception of the Democratic victory in the congressional election of 1958, our measure based solely on control of office hides the general improvement in Democratic voting strength in the state during the last thirty years. An analyst of trends would have to say that the state has become more competitive even though that fact is not reflected in party control of office. Of course, to the extent that closeness of vote does pay off in control of office, it is reflected in our measure. Therefore, recognizing the limitation of failure to reflect unfulfilled trends, our measures of competition will indicate over-all relative party strength, and the cyclical party strength or rate of alternation between the parties.

[4] Closeness of vote as the determinant of competition has been used by Ranney and Kendall (1954) and by Key (1956, page 99).

[5] The sources of information for the electoral history of the offices were the *Congressional Directory* for federal offices, and state blue books for state positions. I am grateful to Professor V. O. Key, Jr. for the use of his electoral files to supplement information not available elsewhere.

COMPETITIVE OFFICE STRUCTURES

The offices used to construct the competitive office structures of the states are the major ones elected by the state at large: governor and U. S. senator, and lesser state administrative positions elected by most states: lieutenant governor, secretary of state, auditor, treasurer, attorney general, and state comptroller. To these has been added a composite measure of the competitive level for congressmen. The composite character of this last measure has a special effect upon the result, and that will be noted below. For each position, then, in each state, we have sought to measure the degree of competition—according to our two basic dimensions—and then all are combined to create a picture of the competitive structure.

The two measures form the axes of the scatter diagram. Along the horizontal axis is measured the over-all degree of competition or percentage of victories for the dominant party—defined as the party with the majority of such victories. Along the vertical axis is measured the rate of turnover in office for the same period, or the percentage of elections in which the winning party differed from the incumbent. Thus the two measures combine to give a reflection of the kind of competition for the office—the more centrally located on the horizontal axis the more competitive an office was in over-all terms; the higher on the diagram the more rapid the rate of turnover; and correspondingly, the lower on the diagram an office falls, the longer the cycles of one-party control, regardless of the degree of over-all competition. The two sloping lines represent the mathematical bounds of possible positions for the two related variables. In Figures 2 through 9, the positions of the offices in the states have been plotted, and lines drawn connecting them. For this purpose we shall ignore the positions of the 12 Southern states. To the extent that they are purely Democratic all of their offices fall at the extreme lower right portion of the diagram. (There are of course exceptions, particularly with respect to federal offices in Oklahoma, Tennessee, Texas, and Florida).

The space on the diagram (see Figure 1) has been divided arbitrarily into three zones, according to the type of competition for an office falling within each area. In the *competitive* zone are those offices which one party has controlled for 70 per cent or less of the elections, and where the rate of turnover has been greater than 30 per cent. In the *cyclical* zone, or area containing offices which have been held by both parties for long and uninterrupted periods of time, are those offices falling below the area of competition. Within the cyclical zone the horizontal base has been extended to include those instances where a party may be decidedly in the minority in over-all terms, but in which it has been able to hold the office for lengthy periods. And finally, the third zone is the *one-party predominant* area, where one party is clearly in command of the office over the time period (over 70 per cent of the elections) and in which the minority has had no lengthy cycles of control.

TABLE I

DISTRIBUTION OF OFFICES AMONG THE STATES ACCORDING TO COMPETITIVENESS

(1914–1958)

	Number of States	Per Cent in Each Zone		
		Competitive	Cyclical	One-Party
Governor	36	50	6	44
Senator	36	42	11	47
Congressman	36	8	61	31
Lt. governor	24	21	25	54
Sec. of state	30	13	30	57
Att'y general	26	11	31	58
Auditor	21	14	34	52
Treasurer	26	15	35	50
Comptroller	5	20	0	80

If we take each office in the 36 non-southern states and determine its zone according to Figure 1, we establish a general picture of the relative difference among the offices. These differences have been gathered in Table I. Both governor and senator were distinctly the most competitive offices of all. But it is apparent that they fell at the extremes, either competitive or one-party, with very few states in which they were cyclical. In those few states in which they were cycli-

cal, as in Rhode Island and Nebraska for governor; Rhode Island, Washington, New Mexico, and New York for senator, they formed parts of state systems which were cyclical for all offices or represent divided party control of the two senate seats. Thus these offices, whenever there was competition within a state, were the ones most likely to move toward the upper center portion of the diagram.

For congress, the greatest proportion of states fell within the cyclical zone. This result is not a product of the fact that the states have had alternating periods of one-party control of the congressional delegations, but is due to the fact that the figure derived here is a composite.[6] The position does show that there was a very low rate of alternation among the parties for congressional seats, which was in turn a reflection of the fact that most congressional districts were one-party in character. The differences among the states illustrate the comparative one-partyness of their congressional districts. For example, Massachusetts, with a 69-31 per cent over-all division in over-all party victories for the period, has had only a little over 5 per cent turnover rate among the parties for congressional seats. The much more evenly competitive state of Connecticut had about the same over-all division of party victories with over 38 per cent party turnover. Apart from Connecticut, the other two states to have their congressional seats fall in the competitive zone were Delaware and Nevada, both states with only a single congressional seat, and thus not subject to the composite effect.

For the minor state offices, the distribution is primarily in the one-party and cyclical zones. There was a tendency for the office of lieutenant governor to be more competitive than the others, as in Ohio and New York. And where the others were competitive, it was usually as part of a general state of competitiveness, as in Utah.

[6] The composite figures for congress were determined by totalling the number of elections and determining the proportions for each party and the proportion of party changes. A special problem arose in determining turnover after the three reapportionments made during the period (1932, 1942, and 1952). Wherever it was possible to trace the district on through this was done. But if there was a substantial change in districting so that turnover became meaningless, the election was ignored for that state and the count was begun again from the time of reapportionment.

The figure for the senate seats is also a composite of two positions. In this case, turnover was determined by observing each senate line independently, then totalling the results.

RANGE OF COMPETITION

The different levels of competitiveness among the major offices of governor and senator and the minor state offices is not unexpected. The real utility of the present measure lies in its ability to discriminate among the states according to the spread between the kind of competition for one office and another. What we have discovered here is a variable in electoral behavior which is the *range of competition*. What the multitude of shapes and spreads among the states in Figure 2–9 shows is that it is far from adequate to describe a state's competitive system solely in terms of a unilinear measure. Nor is it enough simply to add the cyclical dimension. It is as important in measuring competition within a party system to point out the range from one office to another.

A typology of competitive range based on the percentage spread within a state among offices along both the vertical and horizontal axes has been formulated (see Table II). The most narrowly focussed states (I) show little variation in position from office to office. Ten states fall in this group. The second group of thirteen states (II) have their offices fall within a narrow range horizontally, or in terms of over-all competition, but are elongated vertically, indicating that there was great variation among offices in the length of the party cycles. Category (III), consisting of five states, has a medium-broad focus with marked variations in the way in which each office behaved, but not with such wide variations as to make the state appear to be chaotic. For all of the states of the first three categories, it is possible to assign a general competitive position, both with respect to which, if any, party was dominant, and its general extent. The last group (IV) of eight states has such a spread both horizontally and vertically that they have no focus. Their offices range through all

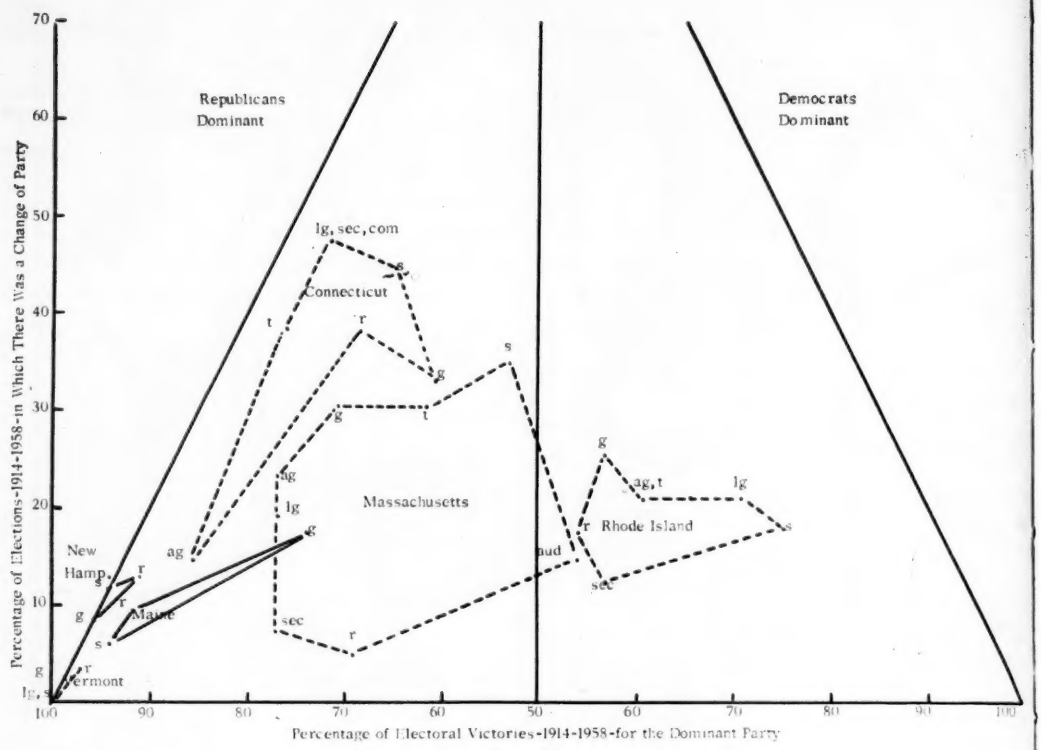
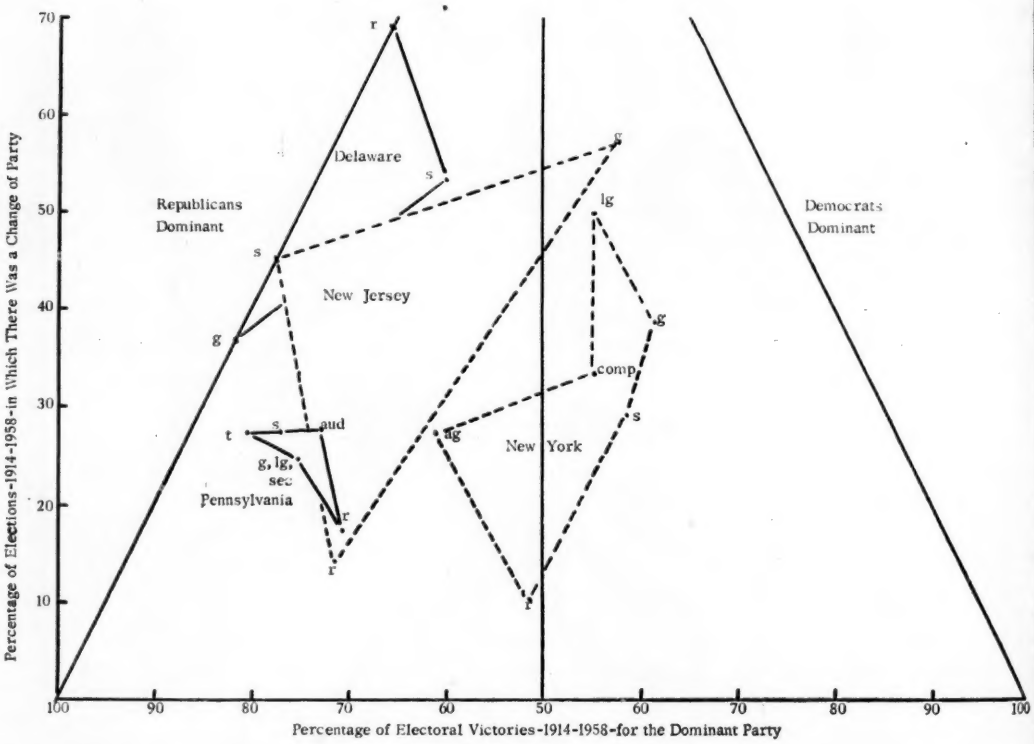

Republicans
Dominant
Democrats
Dominant
lg, sec, com
Connecticut
Massachusetts
Rhode Island
New
Hamp.
Maine
Vermont
Percentage of Elections-1914-1958-in Which There Was a Change of Party
Percentage of Electoral Victories-1914-1958-for the Dominant Party

FIG. 2


Republicans
Dominant
Democrats
Dominant
Delaware
New Jersey
New York
Pennsylvania
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sec
Percentage of Elections-1914-1958-in Which There Was a Change of Party
Percentage of Electoral Victories-1914-1958-for the Dominant Party

FIG. 3

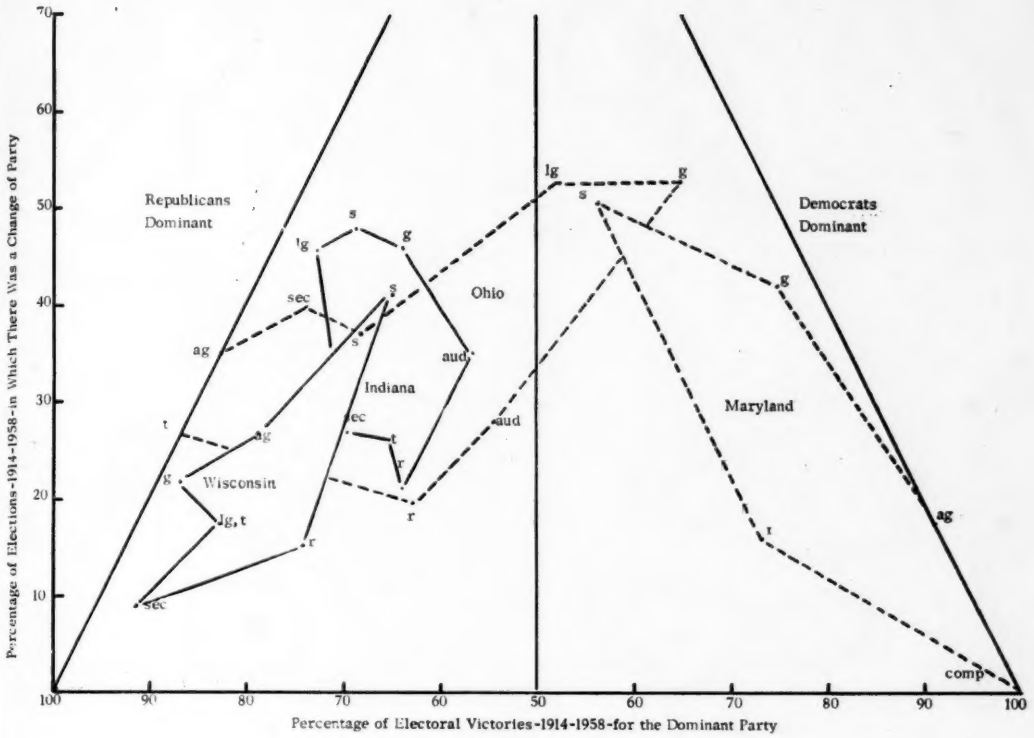

Republicans
Dominant
Democrats
Dominant
Ohio
Indiana
Wisconsin
Maryland
Percentage of Elections-1914-1958-in Which There Was a Change of Party
Percentage of Electoral Victories-1914-1958-for the Dominant Party

FIG. 4

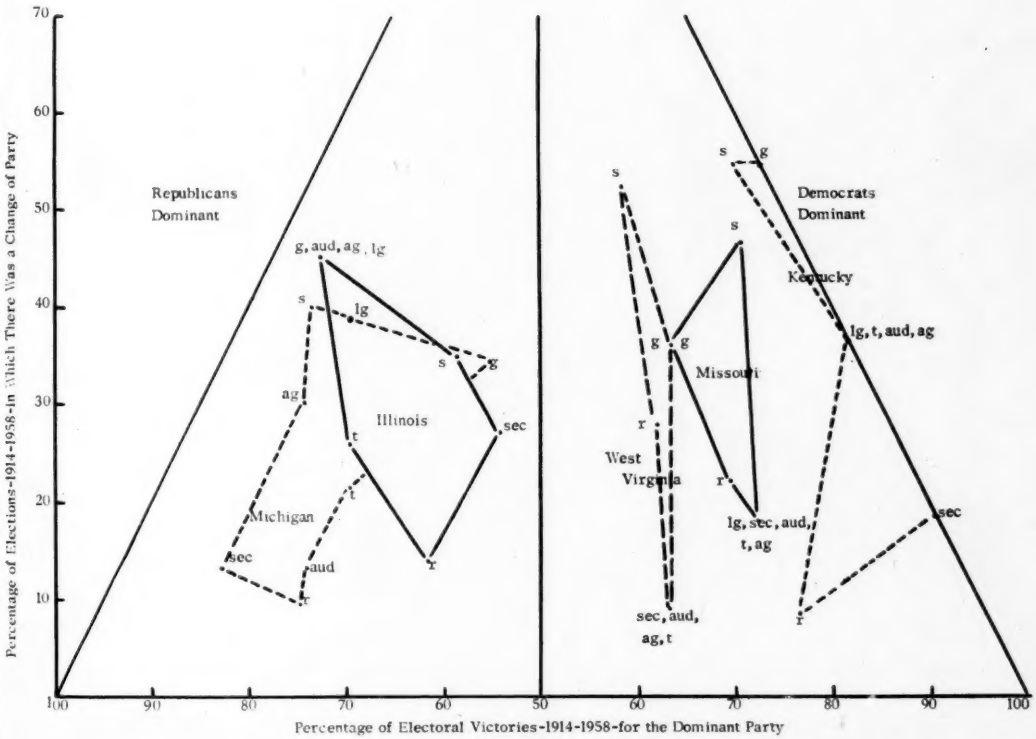
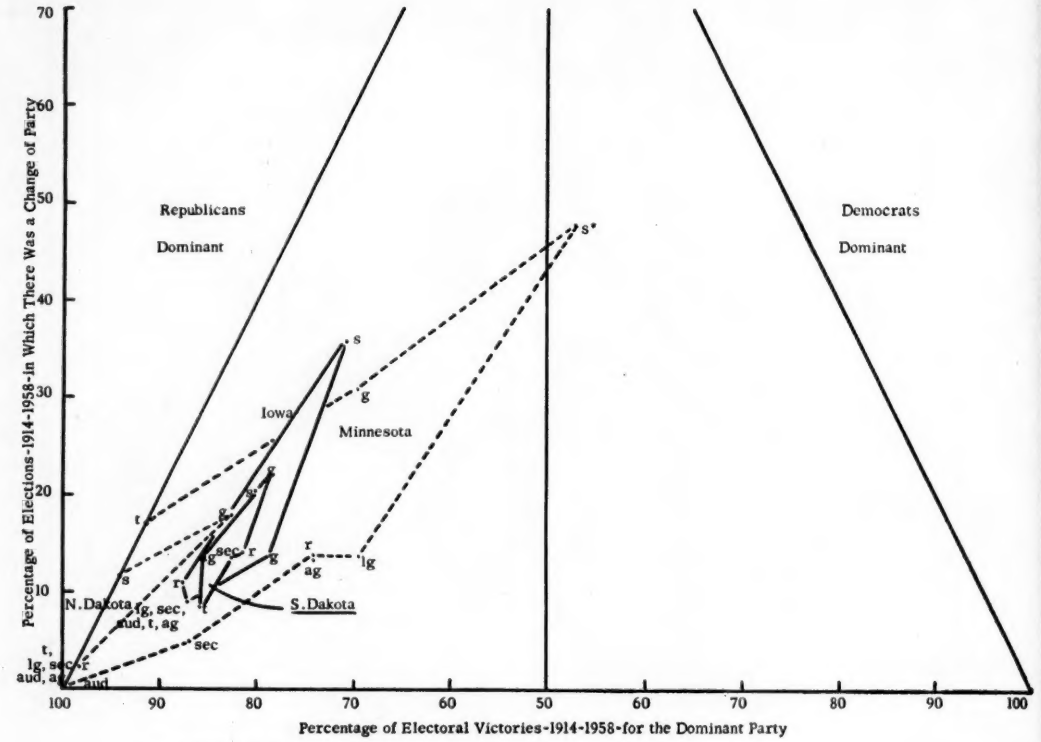

Republicans
Dominant
Democrats
Dominant
Illinois
Michigan
Missouri
Kentucky
West
Virginia
Percentage of Elections-1914-1958-in Which There Was a Change of Party
Percentage of Electoral Victories-1914-1958-for the Dominant Party

FIG. 5



(*All Farmer-Labor included with Democrats in Minnesota)

FIG. 6

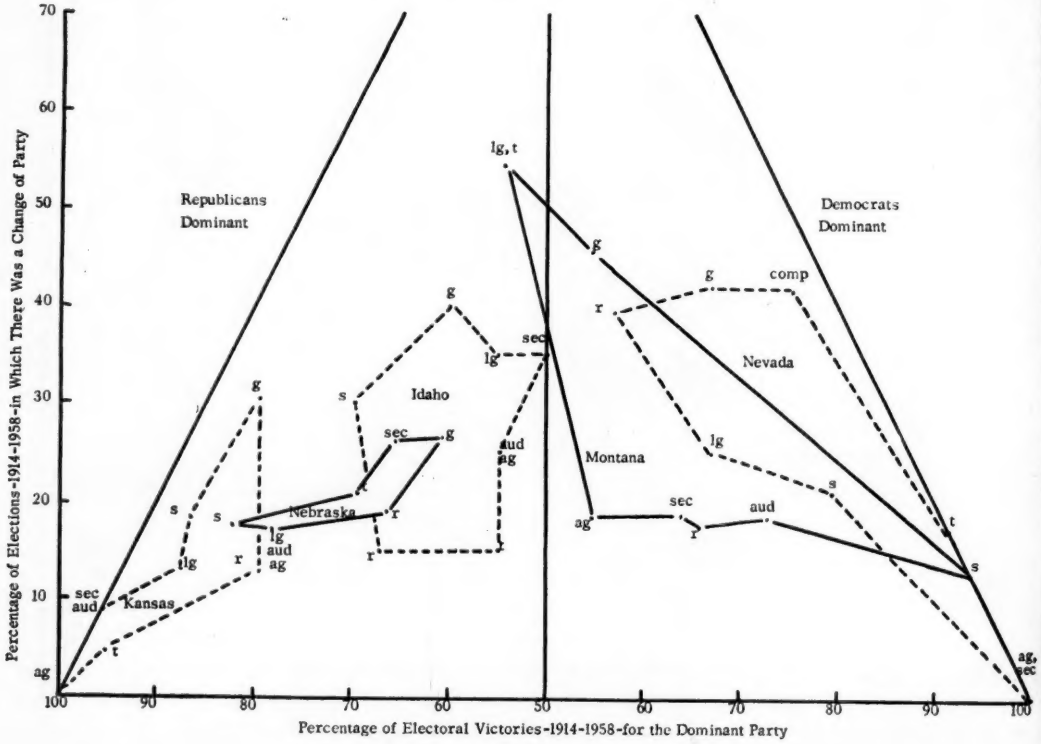


FIG. 7

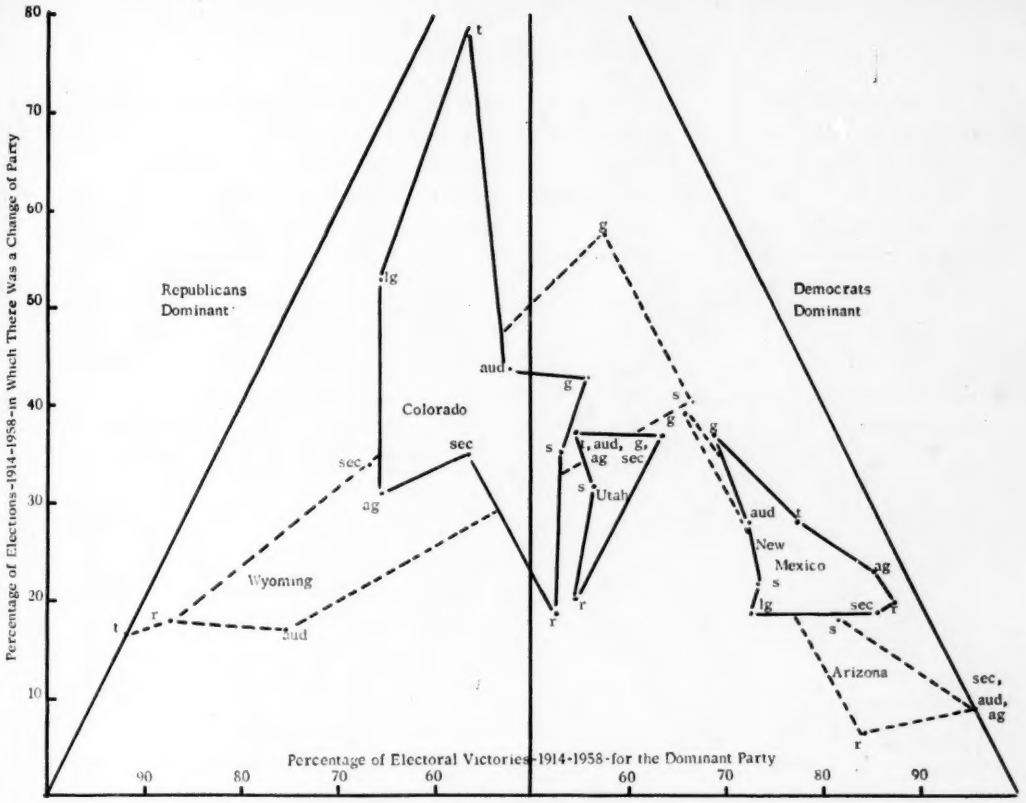

Republicans Dominant
Democrats Dominant
Colorado
Wyoming
Utah
New Mexico
Arizona
Percentage of Elections-1914-1958-in Which There Was a Change of Party
Percentage of Electoral Victories-1914-1958-for the Dominant Party

FIG. 8

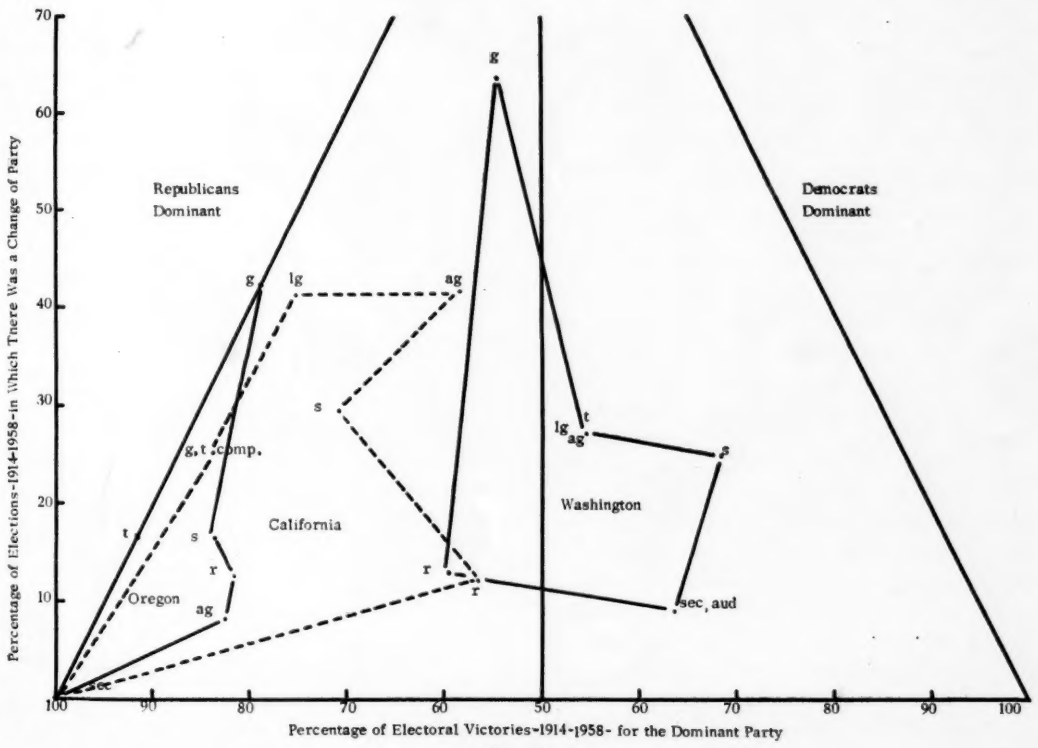

Republicans Dominant
Democrats Dominant
California
Oregon
Washington
g,t, comp.
Percentage of Elections-1914-1958-in Which There Was a Change of Party
Percentage of Electoral Victories-1914-1958- for the Dominant Party

FIG. 9

TABLE II

THE RANGE OF COMPETITION—BASED UPON THE DEGREE OF SPREAD AMONG THE OFFICES OF A STATE IN FIGURES 2 THROUGH 9

I. Narrowly focussed states—maximum horizontal spread of 25%, maximum vertical spread of 20%

Rhode Island	North Dakota
New Hampshire	South Dakota
Maine	Nebraska
Vermont	New Mexico
Pennsylvania	Utah

II. Cyclically elongated states—maximum horizontal spread of 25%, minimum vertical spread of 25%

Connecticut	Illinois
New York	Missouri
Delaware	Iowa
Indiana	Kansas
West Virginia	Idaho
Kentucky	Colorado
	Oregon

III. Medium-broad focus—approximately 30% spread along both axes

Massachusetts	Wisconsin
Michigan	Arizona
	New Jersey

IV. No focus—more than 40% spread along both axes

Ohio	Nevada
Maryland	Wyoming
Minnesota	Washington
Montana	California

three zones of competition, and in some instances, as in Ohio, Montana, and Wyoming, each party has had offices which it tended to dominate. For these states, it is not only impossible to say that one or the other party was dominant, but it is also meaningless to say that they were competitive. In Montana, for example, the Democrats have won practically every senatorial election whereas the Republicans enjoyed an edge in contests for lieutenant governor. And, how is one to state the competitive level of Wyoming, where the Republicans have won almost all the congressional elections and the Democrats most of those for the senate? Here the range of competition was so great that the normal terminology of party theory is of little use.

The four sets of states, grouped according to range of competition, represent in descending order the extent to which a single scale of competition has meaning for the entire state political system. All of the states of narrow focus (group I), with the exception of Rhode Island, New Mexico, and Utah, were strongly Republican. Only Utah falls sufficiently to the center and upper portion of the diagram to warrant the description competitive. The states of group II could be classified as units if we were to eliminate some offices. Or perhaps a better way of stating it would be to say that they could be put into two categories. For example, in West Virginia and Missouri the senate seats were competitive, whereas all other offices were cyclical. In Iowa, the senate was competitive (with a Republican advantage) whereas the other offices were predominantly Republican. In Illinois, governor, auditor, and attorney general were Republican dominant with a high turnover, indicating frequent short run victories for the Democrats. Secretary of state, treasurer, senator, and congressmen were dominantly Republican but with longer cycles for the two parties. Kansas and Oregon were states where most offices were very predominantly Republican, but where there was a high turnover for the office of governor, the one office the Democrats were most successful in capturing, but also one that they were not able to hold for long. In Colorado, with the exception of congress, all offices fell easily within the competitive zone, but some, such as treasurer and lieutenant governor, were extraordinarily competitive in that they had a very high rate of party turnover.[7] Thus within category II the major difference among the offices is in terms of the cyclical strength of the parties. For the states of the less focussed categories (III and IV), no simple statement of competitive structure is possible.

[7] Some of the turnover between the parties may reflect simply rotation of offices among personnel. This is definitely the case in Colorado where the position of state treasurer, as the most competitive office in the United States, is derived from the fact that the same individual tends to rotate that office with the position of state auditor. What appears in this case to be a highly competitive office is in reality one controlled by a very few men who run on party tickets but whose ability to win is not affected by the general party fortunes.

DISCUSSION

As stated at the outset, the conditions of competition in the United States pose some difficulties for the classical two-party theory. We are now in a position to assess the degree of aberration from the assumptions of the theory and to speculate on the reasons for such divergence. In its most simple terms, in the classical party system there ought to be no spread at all between the positions of the offices on the diagrams. A rational electorate perceiving government in terms of party would implement its choices by electing for all offices men of the same party. Using our measure of competition, we have described no state which fits this description. The closest is Vermont, and if we had included the one-party states of the South, they too would fit very closely the assumption that the electorate votes a straight party ticket. And most of the states which approximate unity in their electoral behavior (Category I) are states where one party is decidedly dominant. In effect, in order to achieve one condition of the classical party theory, namely unity of electoral behavior, we have to abandon another element of that theory, i.e., active party competition.

Are there any states which exhibited some unity of behavior and which were also competitive? Only three of the narrowly focussed states were not completely within the one-party dominant zone: Utah, Rhode Island, and Nebraska. Of these, only Utah had no office in the one party zone, and was generally a competitive state. Rhode Island and Nebraska were more definitely cyclical than competitive, in the sense that a more restricted time period would give radically different and more one-party results for their offices. Of the states of category II, Connecticut, New York, and Colorado, despite their great differences in cyclicality, were sufficiently competitive for most of their offices to be called generally competitive.

There is one condition in which voting disparity need not contradict the assumptions of the classical theory, and that is where the voters make a distinction between federal and state office. A preference for one party in national government and another in the state is not unreasonable considering the division of functions between the two. And, as Pennock (1959) has demonstrated, a profit may be derived from this seeming opposition. Therefore, in those states where the competitive range is due to discrepancies in vote for federal and state office, we need not necessarily discard or reformulate the classical system. The states in which the federal voting pattern appeared seriously out of line with the vote for internal office are first, four of the border states: Delaware, West Virginia, Missouri, and Maryland; and three midwestern farm states, Wisconsin, Iowa, and Minnesota. In each of these cases, it was the federal office which was more competitive than those of the state. One may explain this in terms of conscious voter preference for divided party control. Another explanation may lie in the efforts and foci of party organization. In these states, the minority party has gained its greatest advantages from its connections with the national party. Thus the border state Republicans are in a very strategic position with respect to national Republican politics and it may be that their efforts have been concentrated on national office to the neglect of internal affairs. But whatever the reasons for the disparity, for these seven states, the classical theory may be considered descriptive.

Possible explanations for range

It is when the voters disregard party within the same jurisdiction, federal or state, and select officers of opposed parties, that the two-party system loses its validity. And the range of competition found in most of the states indicates that this is a very common occurrence. We have not included control of state legislatures within the outlines of our scheme. But if we had, it is likely that the competitive range would be even greater than that shown. Thus we must go outside the bounds of the two-party assumption to explain competition in the states.

One explanation may be that voters perceive offices differently, particularly within the framework of the party system.

Much of the cyclical character of the minor state offices reflects not only one party control for long periods but also control of the office by a single person. In other words, there is the tendency for the electorate (and the party organizations) to treat the lesser state offices as though they were careers, and to permit a person, regardless of the flow of party fortunes, to retain the seat. Thus there is a built-in lag in party turnover for these positions.

Another explanation may lie in different electorates for each office. For most of the offices we have included on our charts, there is no formal distinction among their electorates; they are elected by the state at large. But the congressional seats are apportioned and districted with the result that for most of the states competition for congress is much lower than for any other office. Undoubtedly the inclusion of the same information on state legislatures would produce a similar result. Here voting behavior is less of a factor in producing the different result than electoral engineering.[8]

The discrepancy in levels of competition may lie in mechanical factors. There is, for example, a tendency for the number of ballots cast for the lesser offices to drop off even in the same election. If the vote was close, and if the voters not completing their ballots were unevenly distributed among the parties, such behavior would result in a divided election. The number of "ifs" in this explanation make it unlikely that it can account for many of the discrepancies. Election timing, however, is a mechanical factor which probably does have an effect upon party control of office. Although all of the offices selected for examination were elected in November of even numbered years (except New Jersey and Kentucky), their cycles varied in some states, some elected every two years and others every four, or with overlapping four year cycles. The most common system is to have all elections coincide, but we find overlapping four year cycles in Oregon and Pennsylvania. In Minnesota, all offices were elected for two year terms except the auditor who had a four year term. In Illinois, all offices were on a four year cycle except the treasurer who had a two year term. And in Indiana the governor and lieutenant governor are elected for four years and the other state offices for two. Of course in every state the congressional seats are elected every two years while the two senate seats are elected on separate six year cycles. Such differences in election timing permit considerable variation in electoral reaction. For one thing, popular sentiment shifts in time from party to party causing differential control of office. Then again, the normal turnout drop in midterm elections is such that it has a marked effect on the results, which in turn will have a differential effect if all offices are not voted on at the same time. However plausible this explanation, it is clear that it is far from the whole answer, since the states in which it was relevant were not the ones with markedly wide competitive ranges. The great ranges in competition found in states such as California, Wyoming, or even Massachusetts cannot be explained by election timing.

Probably the fundamental reason for the range of competition among offices in a state is a rejection of the very notion of party. Whether this is due to a failure of the parties to create organizations encompassing all of the offices, or to an unwillingness of the electors to think in party terms, it is certain that non-party considerations play a major part in American electoral decisions. The competitive range we have described is in effect a measure of the failure of party systems to develop. And, as we have seen, there is considerable difference from state to state in the range of competition. At the extreme, the states with no focus are those which, on the basis of other evidence, have had weak or meaningless party organizations. A general inference has been that the western states have less well organized parties than those of the east. The inclusion of Montana, Nevada, Wyoming, Washington, and California in the non-focussed group supports that inference. For practically all of the period, California has used "cross-filing" in its nominating primaries

[8] As March (1957) has shown, in attempting to apply the cube law to congressional elections, the division of party strength in collective bodies is not a simple linear reflection of the voting proportions.

with disastrous effects upon the sense of party. And all studies of the California parties emphasize the weakness of their organizations (McHenry, 1948). Washington, another non-focussed state, is the home of the "blanket" primary, which is an anathema to proponents of cohesive party organization. Whether these formal modes of carrying on the major party function of nominating candidates are cause or effect, they reflect situations in which the non-party approach to politics has been dominant. Personal politics take the place of party politics. And where the impact of party label is less important in deciding elections, we must be more cautious in applying the classical party theory to political decisions.

CONCLUSIONS AND IMPLICATIONS

The establishment of differential ranges of electoral competition has some immediate implications for the study of political behavior. In the realm of party theory, it is important that we develop some bases for explaining politics which are free of the pure government-opposition framework. In the current literature, there is only one alternative which fits all of the conditions we have found, and that is the elite theory of C. Wright Mills (1956). Mills' concept simply denies the relevance of parties and competition to policy decisions. Thus differences in competitive levels have no importance since party competition itself is but a facade. But since the empirical basis for the elite approach is even shakier than that of the classical party theory (see Dahl, 1958), we are still left with the need for a realistic party theory.

The revelation of the varieties of behavior among state electorates presents a challenge to the student of voting behavior. Most of the survey studies have dealt solely with national elections, and presidential elections in particular (Campbell, Gurin, & Miller, 1954; Berelson, Lazarsfeld, & McPhee, 1954). The enormous discrepancies in voting reaction to different offices implies that the scope of such studies should be broadened, especially if they are to be useful in understanding collective behavior generally. The regional differences in competitive structures, furthermore, indicate rather important variances in voting outlook which may be obscured in national samples which ignore the state or political unit of the respondents. As Key and Munger (1959) have pointed out, survey methods ignore local political conditions. Our evidence shows the great importance of the state as a unit in voting analysis, for even states within the same geographic region have markedly different ways of reacting to parties and offices.

And most important of all, the differences found in the range of competition make necessary a reconsideration of our notions of party organization. Actually very little general work has been done on American party organization other than legalistic descriptions. Whatever has been done in terms of general empirical analysis has been put in terms of more or less cohesion, or more or less factionalism within the parties. To the extent that there is a generalization, it has been assumed that the greater the level or degree of inter-party competition, the greater the degree of party cohesion and discipline. But since we have demonstrated the difficulty of applying a unilinear notion of competitiveness in most states, we must seek another relationship between competition and party organization.

An hypothesis which appears to be useful in approaching parties is that party organization (in terms of acts of co-operation, unity, cohesion) will revolve about the different levels of the range of competition. Or, in other words, offices which are elected under similar electoral conditions will tend, in terms of the actions of candidates and their supporters, to coalesce within a party. Contrariwise, offices elected under different circumstances than others will tend to develop independent organizations. Thus, if a state is highly competitive for federal offices and is dominated by one party for state positions, we would expect two distinctive organizational foci to develop, in contrast to a unified organization in a state which was highly competitive for all offices. Or, if a number of lesser state offices tend to be elected in longer cycles than the top positions, we would expect that the incumbents of these offices would be, other things equal,

less concerned about acting cohesively with the rest of the party. Thus the structure of competition within a state may also be a picture of the structure of party organization.

A final note should be made about the implication of these findings for the study of politicians and their careers. For the competitive structure of a state also serves to define one major aspect of the opportunity structure. From the politician's point of view, variations in competition are a statement of his chances for advancement to a particular office. In turn, the party's recruitment patterns are determined by what are the party's chances of winning office. Thus the framework of leadership recruitment and selection is formed for each party by its position within the competitive structure.

In conclusion, then, we have measured competition for public office in the American states by placing a number of offices on a two-dimensional scale according to over-all and cyclical competitiveness in the period from 1914 to 1958. The dispersal of offices within a single state then provided us with an additional dimension, namely the range of competition. Since this dimension of competition is not compatible with our current conceptions of a two-party system, it is important that party theory be more directly related to empirical conditions of American politics.

REFERENCES

Amer. Political Sci. Assn., Committee on Parties. *Towards a more responsible two-party system.* New York: Rinehart, 1950.

Berelson, B. R., Lazarsfeld, P. F., & McPhee, W. N. *Voting.* Chicago: Univ. Chicago Press, 1954.

Campbell, A., Gurin, G., & Miller, W. E. *The voter decides.* Chicago: Row Peterson, 1954.

Dahl, R. A. A critique of the ruling elite model. *Amer. Pol. Sci. Rev.*, 1958, 52, 463–469.

Diamond, Martin. Review of A. Downs, *An economic theory of democracy. J. Pol. Economy*, 1959, 67, 208–211.

Downs, Anthony. *An economic theory of democracy.* New York: Harper, 1957.

Golembiewski, R. T. A taxonomic approach to state political party strength. *Western Pol. Quart.*, 1958, 11, 494–513.

Key, V. O., Jr. *Southern politics.* New York: Alfred A. Knopf, 1949.

Key, V. O., Jr. *American state politics.* New York: Alfred A. Knopf, 1956.

Key, V. O., Jr., & Munger, F. Social determinism and electoral decision: The case of Indiana. In E. Burdick & A. J. Brodbeck (Eds.), *American voting behavior.* Glencoe, Ill.: The Free Press, 1959.

Key, V. O., Jr., & Silverman, C. Party and separation of powers: A panorama of practice in the states. *Publ. Policy*, 1954, 5, 382–412.

Lockard, Duane. *New England state politics.* Princeton, N. J.: Princeton Univ. Press, 1959.

MacRae, Duncan, Jr. The relation between roll call votes and constituencies in the Massachusetts House of Representatives. *Amer. Pol. Sci. Rev.*, 1952, 46, 1046–1055.

March, James G. Party legislative representation as a function of election results. *Publ. Opinion Quart.*, 1957, 21, 521–542.

McHenry, Dean. The pattern of California politics. *Western Pol. Quart.*, 1948, 1, 45–52.

Mills, C. W. *The power elite.* New York: Oxford, 1956.

Pennock, J. R. Federal and unitary government—disharmony and frustration. *Behav. Sci.*, 1959, 4, 147–157.

Ranney, A., & Kendall, W. The American party systems. *Amer. Pol. Sci. Rev.*, 1954, 48, 477–485.

Rogers, W. H. Some methodological difficulties in Anthony Downs' *An economic theory of democracy. Amer. Pol. Sci. Rev.*, 1959, 53, 483–485.

Schattschneider, E. E. *Party government.* New York: Rinehart, 1942.

Schlesinger, J. A. A two-dimensional scheme for classifying the states according to degree of interparty competition. *Amer. Poli. Sci. Rev.*, 1955, 59, 1120–1128.

Schlesinger, J. A. *How they became governor.* East Lansing, Mich.: Pol. Res. Studies, No. 3, Governmental Res. Bureau, 1957.

Turner, Julius. Primary elections as the alternative to party competition in "safe" districts. *J. of Politics*, 1953, 15, 197–210.

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Often scientists talk about the same things with different vocabularies and from different points of view. To simplify common patterns of analysis used in the study of groups, this author presents a scheme for conceptualizing the basic variables in such research on face-to-face social interaction. Major dimensions of observed behavior are delineated under the headings of form and content of interaction for both personal and interpersonal behavior. This analysis is suggested as a framework for organizing ideas in this area of research.

THE DIMENSIONS OF SOCIAL INTERACTION[1]

by A. Paul Hare

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In reviewing the literature on social interaction in small groups which has been published during the past 50 years (Hare, in press; Hare, Borgatta, & Bales, 1955; Strodtbeck & Hare, 1954), it became apparent that many of the authors were talking about the same things but with different vocabularies and from different points of view. A conceptual scheme was developed to include the dimensions of social interaction that appeared most frequently in the literature.

The literature on social interaction covers three general classes of observations. In some cases, the focus is on *inter*personal behavior such as cooperative problem solving; in other cases, it is on *intra*personal behavior as evidenced in tension or anxiety. In still other cases, the focus is on aspects of *individual performance*, which may characterize an individual whether he is alone or in a group. In this paper, intrapersonal behavior and individual performance will be combined, leaving two very general categories—*interpersonal behavior* and *personal behavior*.

Although many different category systems have been used in describing interpersonal behavior, the categories of interaction given in Figure 1 appear to represent the major "dimensions" of observed behavior. These dimensions refer to both the form and the content of interaction.

[1] Presented as a paper at the Meetings of the American Sociological Society, Seattle, September, 1958.

The form of interaction is less specific than content and is more easily recorded. As one approaches a group from the "outside," the first aspect of interaction which becomes apparent is the communication network (i.e., who speaks to whom) and next the amount of interaction carried by each of the channels. For a closer analysis of what is going on in the group, one needs some content categories. Here the most frequent division is between content directed primarily towards the solution of task problems vs. content directed primarily towards the solution of social-emotional problems. Within the social-emotional area, the categories of control and affection represent the predominant types of behavior, while in the task area there is less consensus on the "typical" problem-solving categories. The minimum set of categories parallels the steps in the scientific method, namely of observation, hypothesis formation, and the testing of hypotheses.

Each of the major categories in Figure 1 can be identified as follows:

Form: *Communication network*. The channels of communication between group members. *Interaction rate*. The frequency of interactions, sometimes represented by the number of contributions, sometimes by the relationship between the number and duration of contributions, i.e., action and silence, and sometimes by the number of contributions times the average duration of each, i.e., total talking time.

Content: *Task behavior*. Interaction di-

Form	Communication network
	Interaction rate
Content	Task behavior a. observe b. hypothesize c. propose action
	Social-emotional behavior a. control b. affection

FIG. 1. A Paradigm for the Analysis of Interaction.

rected toward the completion of group or individual tasks. The minimum number of categories would include observing, hypothesizing, and formulating action. *Social-emotional behavior.* Interaction directed primarily toward the relationships between group members which form the basis for problem solving. The minimum number of categories would include control and affection.

Each of these categories can be used at the level of personality, behavior, and role. At the level of personality they represent *tendencies to act* and at the level of role they represent *expectations for behavior.*

OUTPUT AND INPUT: FORM

The description of a person from the interaction point of view includes not only how he acts toward others (output), but also how others respond to him (input). For this reason, the minimum number of actors in a unit of interaction is two, the minimum number of acts is two (one action and one reaction), and the minimum number of time periods in which interaction occurs is two (Bales and Slater, 1955).

The output and input characteristics for both form and content of interaction can be considered with varying degrees of complexity. For the communication network one can record simply the total number of channels for outgoing messages and incoming messages. At the other extreme, one can specify the extent to which each channel from a subject to every other member of the group is open for output or input. Similarly, the interaction rate can be described simply as the total number of acts an individual gives and the total number he receives, or the output and input rate can be given for each channel in the communication network.

Although it is possible to differentiate output and input for the communication network, particularly in experiments in which the communication network is the major variable, the correlation between output and input for a given channel in most situations is very high. The person who speaks most often in a group is usually the one who is spoken to most often. In those cases where the correlation is high, specification of the input characteristics may add little to the description of the interaction process.

This type of reduction in the number of experimental "variables" occurs over and over again in those experiments which use comprehensive category systems for recording the interaction process. Since relatively few categories of behavior are appropriate in any given situation, most of the interaction tends to take place within two or three categories. As a result the remaining categories are either correlated with those or appear too infrequently to yield reliable measures.

OUTPUT AND INPUT: CONTENT

For the analysis of behavior where one may wish to consider several categories of content simultaneously, the characteristics of an individual's interaction for either output or input may be described by a set of *interaction profiles* similar to those used by Bales (1950). If only one dimension or area of content is to be considered at a time, a graphic plot may be used to advantage, with one axis representing output and the other input. (See Figure 2). The explicit recognition of the distinction between input and output may help to clarify some theoretical issues, especially in the construction of typologies.

In the description of behavior tendencies in the area of control, for example, the vertical axis might represent the behavior desired from others ranging from *yes* he wants to be controlled by others to *no* he does not want to be controlled by others. The horizontal axis could then represent the desired behavior toward others ranging from *yes* he wants to control others to *no* he does not want to control others.

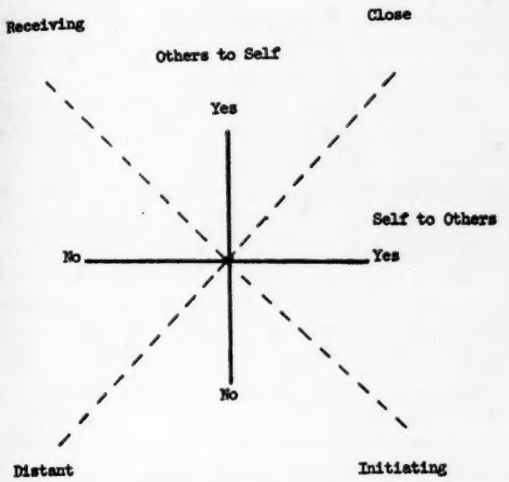

FIG. 2. Representation of Input and Output in a Two-Dimensional Plot.

The individual who wants strongly either to control or be controlled has been called *authoritarian*. On the other hand, the individual who wants neither to control nor to be controlled may be said to desire a situation in which there is distance between group members. In the extreme, this kind of independence has been called *anarchism*.

On the opposite diagonal are individuals who vary from wanting to control but not be controlled, the *dominators*, to those who want to be controlled but do not want to control, the *submitters*. In the center are those individuals who want to both control and be controlled in a moderate degree, a type of behavior which tends to be called *democratic*. All other combinations of tendencies to give or receive control can be represented as points in this two dimensional space.[2]

In the description of personality, the two axes in Figure 2 would represent the tendencies to give and receive which the individual brings to the situation. In the description of *role*, the axes would represent expected behavior, the duties (output) and the rights (input) associated with each role. The expectations for behavior would vary along each axis from behavior which is required to that which is prohibited.

[2] Schutz (1958) has developed personality tests which are based on this kind of two-dimensional representation. Leary (1957) presents a comparable point of view although input and output are not as clearly differentiated.

PERSONAL BEHAVIOR

In addition to the interpersonal categories, there are dimensions of personal behavior which also play a part in the activity of a group. The personal categories include those dimensions usually associated with personality such as intelligence, social sensitivity, and adjustment, as well as aspects of the individual's biological nature such as age, sex, and physical strength. These categories, like the previously discussed interpersonal categories, can also be used to describe "tendencies" within the individual and "expectations" for the role he is to perform.

Although the mean or average behavior in each category of personal and interpersonal behavior is stressed in the present formulation, this should not obscure the fact that the mean may not be as important for the prediction of behavior in a particular instance as some measure of the extent of variation and the conditions under which the variation occurs.

PREDICTING BEHAVIOR FROM PERSONALITY AND ROLE

To predict an individual's behavior in one of the form or content areas, one could first indicate on the plane a point representing

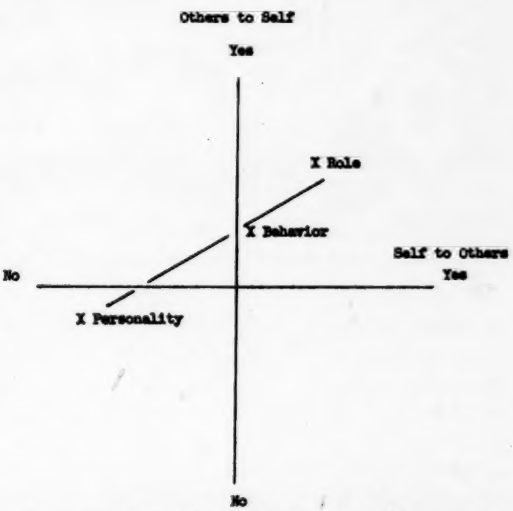

FIG. 3. The Relationship Between Personality, Behavior, and Role on One Dimension.

the tendency in the personality (See Figure 3) and also a point representing the expected behavior called for by the individual's role in the group. The actual behavior would then lie somewhere along the line between the personality and role.

In a ceremony, such as a marriage, the actual behavior would be very close to the role. At the other extreme, when role is not well defined, for example, the role for a patient in a mental hospital ward, the behavior would be close to the tendencies in the personality. Since in many personalities and in many roles one of the content areas may be more salient than the others, an individual's behavior may be predominantly in *one* of the content areas.

VARIATIONS IN INTERACTION RATE

The characteristic of an individual's interactive behavior most frequently reported in the literature is his interaction rate. An individual's interaction rate can vary while the content of interaction remains relatively stable. In most situations, the interaction rate of a single group member is related to the rates of other members of the group as well as to his own personality.

An increase in the interaction rate of an individual may be associated with activity in any one of the content areas. In some experiments, an increase in interaction has been found to be associated with attempts to control a deviant member (Schachter, 1951). In other research, a high interaction rate is correlated with task success (Strodtbeck, 1954). In still other studies, as for example in the observation of working girls doing piece work which does not require control, the interaction rate is highly correlated with affection (Homans, 1950).

CHOICE BEHAVIOR AND COHESIVENESS

The criteria which individuals use in making interpersonal choices may fall into any of the content areas. However, in social research, subjects are often asked to designate group members whom they would like to work with (a criterion which appears to combine task ability and control), and whom they would like to play with (a criterion which appears to be primarily affection). Since individuals who desire to be close to people will choose others who prefer closeness, there is evidence that "birds of a feather flock together." However, individuals who like to initiate tend to choose those who like to receive so that it is also true that "opposites attract."

Groups containing a large number of mutual choices on either "work" or "play" criterion are often said to be highly "cohesive" in that they will "stick together" longer than groups in which there are few mutual choices. Although groups are often referred to as being highly "cohesive" without specifying the basis on which the choices were made, the criterion is important. Subjects who have chosen each other because they like to "work with" each other should be more productive than those who have chosen each other because they like to play together, provided, of course, that the task calls for the type of control relationships they prefer. A group composed of anarchists who had chosen each other could not be expected to do well on a task requiring authoritarian relationships.

The importance of the criterion on which choice is based is not always evident in the literature since many subjects will make the same choices regardless of the criterion which the experimenter suggests. In some cases, the multiple choice is justified since there are "great men" who are actually high on all criteria (Borgatta, Couch, and Bales, 1954), but in other cases it appears that the subject has his own preferred criterion for choice and will use the same one no matter what the experimenter suggests (French, 1956). That is, a subject with a salient need for affection would always choose others whom he expected to satisfy his affectional need whether the situation called for affection, or some other predominant category of behavior.

The approach to the analysis of interaction in small groups outlined in this paper is suggested as a framework for organizing ideas. The elements included in the theory have been found to be important sources of variation in social behavior in a variety of situations. However, the prediction of behavior in any specific situation may require knowledge of only a few of these elements or, on the other hand, an elaboration and

differentiation of these concepts well beyond the present level of analysis.

REFERENCES

Bales, R. F. *Interaction process analysis: A method for the study of small groups.* Cambridge, Mass.: Addison-Wesley, 1950.

Bales, R. F., & Slater, P. E. Role differentiation in small decision making groups. In T. Parsons, R. F. Bales, et al., *The family, socialization, and interaction process.* Glencoe, Ill.: Free Press, 1955.

Borgatta, E. F., Couch, A. S., & Bales, R. F. Some findings relevant to the great man theory of leadership. *Amer. sociol. Rev.*, 1954, 19, 755–759.

French, Elizabeth G. Motivation as a variable in work-partner selection. *J. abnorm. soc. Psychol.*, 1956, 53, 96–99.

Hare, A. P. *Handbook of small group research.* Glencoe, Ill.: Free Press, (in press).

Hare, A. P., Borgatta, E. F., & Bales, R. F. *Small groups: Studies in social interaction.* New York: Knopf, 1955.

Homans, G. C. *The human group.* New York: Harcourt, Brace, 1950.

Leary, T. *Interpersonal diagnosis of personality.* New York: Ronald Press, 1957.

Schachter, S. Deviation, rejection, and communication. *J. abnorm. soc. Psychol.*, 1951, 46, 190–207.

Schutz, W. C. *FIRO: A three-dimensional theory of interpersonal behavior.* New York: Rinehart, 1958.

Strodtbeck, F. L. The family as a three person group. *Amer. sociol. Rev.*, 1954, 19, 23–29.

Strodtbeck, F. L., & Hare, A. P. Bibliography of small group research: From 1900 through 1953. *Sociometry*, 1954, 17, 107–178.

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It is an erroneous impression, fostered by sensational popular biography, that scientific discovery is often made by inspiration—a sort of *coup de foudre*—from on high. This is rarely the case. Even Archimedes' sudden inspiration in the bathtub; Newton's experience in the apple orchard; Descartes' geometrical discoveries in his bed; Darwin's flash of lucidity on reading a passage in Malthus; Kekule's vision of the closed carbon ring which came to him on top of a London bus; and Einstein's brilliant solution of the Michelson puzzle in the patent office in Berne, were not messages out of the blue. They were the final co-ordinations, by minds of genius, of innumerable accumulated facts and impressions which lesser men could grasp only in their uncorrelated isolation, but which—by them—were seen in entirety and integrated into general principles. The scientist takes off from the manifold observations of predecessors, and shows his intelligence, if any, by his ability to discriminate between the important and the negligible, by selecting here and there the significant stepping-stones that will lead across the difficulties to new understanding. The one who places the last stone and steps across to the *terra firma* of accomplished discovery gets all the credit. Only the initiated know and honor those whose patient integrity and devotion to exact observation have made the last step possible.

HANS ZINSSER, *As I Remember Him*

The psychoanalytic situation, with its private relationship between analyst and patient, is difficult to study experimentally and describe in quantitative terms. The intricacies of the relationship, the responsibilities of the analyst to the patient, and other such uncontrolled variables all interfere with attempts to create precise conditions for experimental study. Described he e is an experiment to control for one feature of the analytic situation, the effect on the patient of the analyst's presence, in an effort to quantify our knowledge in this important area.

EXPERIMENT ON THE EFFECTS OF AN OBSERVER'S PRESENCE ON THE IMAGO SYSTEM DURING PSYCHOANALYTIC FREE-ASSOCIATION[1]

by Kenneth Mark Colby[2]

San Francisco Psychoanalytic Institute

INCLUSIVE DESCRIPTION OF FREE-ASSOCIATION IN THE PSYCHOANALYTIC SITUATION

The term

MISNOMERS can still be useful. In psychoanalysis, we recognize that the process termed "free-association" is not free in the sense of being indeterminate, nor is it entirely free of censorship, nor is it always associative. But the term has become so well embedded in our vocabulary that it would be easier to expand its meaning than to change the term itself. Scientific terms can live on long after original meanings have been abandoned and new ones formulated. In communicating with one another, psychoanalysts know what the term "free-association" encompasses and as long as empirical referents of a term are understood, its descriptive aptness is only ornamental.

In the clinical psychoanalytic situation, a patient is given some simple instructions. He is asked to lie on a couch, behind which sits the analyst, and to describe verbally everything he observes entering his conscious awareness. Everything includes everything. He is requested not to exclude any conscious element from his continuous description. Anyone who attempts this task of inclusive description soon realizes that it is, in fact, impossible. It can only be an ideal striven towards with fluctuating degrees of successful approximation. During observation of one's conscious contents, a variform commixture of perceptions, memories, feelings, images, etc. swarm into awareness at a rate far greater than can be matched by verbal description. In describing to another person, some sort of selection must operate to make the descriptions communicatively meaningful. Otherwise the process becomes the chaos of a rapid sequential listing of namings (actually observable clinically when certain patients attempt to mock the instructions regarding free-association). A describer of free-association is aware that an observer is trying to make sense out of what is heard and the describer attempts to help him by selecting and organizing what is said. Hence a major factor in regulating inclusive description of free-association is that verbalization represents communication to another person, who is an immediately present observer. Since a relation between these two persons exists, we know that the describer will select items for description in accordance with the view

[1] This investigation has been aided by a grant from Foundations' Fund for Research in Psychiatry.

[2] I am grateful to Quinn McNemar and Christoph M. Heinicke for their generous counsel.

of himself he wishes to present in this relation at a given time.

Besides not being free, free-association in clinical sessions is not always associative in the expected convergent or divergent sense. There are stretches of time when the patient describes events of the previous day in chronological order or when he reports a complete episode, excluding as too distracting many associations which occur to him during the recounting. Still, we term the over-all process "free-association" and I shall use the term in this sense. When a person attempts to observe the contents of consciousness and describe them to an observer in the psychoanalytic situation, I will term this process "inclusive description."

Its use

A patient inclusively describing free-association is a sender of information through messages which are sequences of symbols selected from a code. We assume that a patient has undergone some experience, a pattern of events in time, and that this pattern has become encoded in the event-structure of the psychic apparatus. Imagic or linguistic representations from this structural code appear in consciousness and are then transmitted in speech in the form of sound wave signals to a receiver, the analyst, who, using his own structural code, decodes the signals into messages containing meaningful information. In these messages, the analyst searches for patterns and regularities which allow him to make useful inferences about the nature of the patient's structural code and its operations.

An analyst first makes use of the patient's inclusive description as a major source of observational data. (Here I will omit consideration of another but lesser source, non-verbal signs and signals.) An analyst's observations consist of messages decoded from inclusive description which form the basis of inferences about what is taking place in the patient's intrapsychic activity. It is from these inferences that he attempts to formulate explanatory generalizations, initially about an individual person and ultimately about the general class of persons.

At what he considers appropriate moments, an analyst verbally intervenes in the patient's flow of association and description. He sends certain messages to the patient which represent a selective input into the patient's structural code. The patient's response, inclusive description of his free-association subsequent to this input, is then used by the analyst to evaluate effects of the intervention. Thus a second use of inclusive description consists of the observational assessment of the effects of selective inputs. Inclusive description of free-association constitutes an empirical source for the initial formulation of hypotheses and for ultimate testing of these hypotheses.

Three questions

Years of clinical experience with inclusive description have not yet resulted in process laws, i.e., tested and dependable generalizations. Nor should we expect it to since clinical psychoanalytic treatment concerns itself primarily with technological problems and not with the aims of pure science.

To an ordinary observer, inclusive descriptions of free-association appear so varied, so meandering, so random, so disconnected that it would seem hopeless to produce any order from them. But a scientific observer realizes this sort of chaos exists for all observation. The ordering problem consists of breaking up observational phenomena into elements of some conceptually selected system and then reconstituting the phenomena in terms of this system. Thus, a psychoanalytic observer making a pure-science investigation of inclusive description of free-association would attempt to (a) describe a system or process under observation, (b) measure regularities in the system or process, and (c) deliberately intervene to effect predictable changes in the system or process.

These attempts can be formulated as three questions: (1) What system or process in inclusive description shall we select to observe? (2) How can we measure the system or process? (3) How is the system or process predictably affected by the analyst?

These questions are of importance for psychoanalytic theories of psychology, psychopathology, and treatment. Psychoanalytic theory consists of a body of propositions not only about psychic illness and its treatment but also about human psychic functioning in general. These propositions range from high-level theoretical assumptions to low-level empirical hypotheses. The usefulness of the assumptions depends on empirical confirmation of hypotheses deductively connected with them. Since the body of psychoanalytic propositions is so dependent on observation, it is important that inclusive description in the analytic situation be exhaustively studied.

In this paper, I shall attempt answers to the three questions posed earlier. They are theoretically important questions and I hope to show that they are empirically decidable questions, having discoverable answers.

Analysts with clinical experience could suggest hunch hypotheses about the kind of answer reasonable to expect to the first question.

Clinical hypotheses

A clinical psychoanalyst observes the flow of inclusive description for several hours a day for many years. He is interested in the meaning content of the messages he decodes, and he searches for various sorts of operational correlations in the meanings being expressed. In attempting to answer the first question—What system or process in inclusive description shall we select to observe?—a clinician could propose at least three content-areas of repeated importance in clinical psychoanalytic treatment: (1) the relation between patient and analyst, (2) the patient's concerns and conflicts with significant other persons in his life, and (3) childhood conflicts involving parents, siblings, or other relatives. Each of these areas involves a patient's relations to significant other persons, to personages important to him and with whom he has time-enduring affectively-valued involvements. Clinical experience suggests the hypotheses that it is relations to these personages which constitute essential (but not the only essential) ingredients of inclusive description, and that descriptions of these relations are influenced by the ongoing relation to the observing analyst.

While a clinician would propose these impressions as hypotheses with a high degree of probability, it is not easy for him to accurately document observations supporting them. Also, in clinical sessions, he is so busy with technical tasks of simultaneously evaluating and interacting with a variety of events that he cannot conduct a controlled test of a hypothesis. He may gather supporting evidence which tends to raise degrees of plausibility, but additional methods must be used to record observations, collect data, and test hypotheses.

It has always been difficult to obtain accurate observation records of the events taking place in clinical sessions. Written records are notoriously full of distortions and omissions. One reliable way of recording observations which others can study and which we can restudy at leisure is to record the session on tapes. Once taped, the totality of inclusive description in a session can be played back and the data selected for collection can then be extracted and notated in writing or in some shorthand form.

In addition to the observational problem in clinical sessions, there is the problem of therapeutic obligation. A clinical analyst undertakes a responsibility towards his patient which can interfere with conditions necessary to test hypotheses. To experimentally test hypotheses, a group of variables must be controlled while other isolated variables are studied. But in clinical sessions there are often unexpected therapeutic demands made on the analyst, and his responding interventions may of necessity be of a nature which ruin controlled experimental conditions.

Finally, a major difficulty in clinical sessions is that we have not yet developed a method of measurement or any standard way of weighting the significance and reliability of our observations. We have the clinical impression that certain qualitatively definable interactions take place in the analytic situation, but the frequency or the extent of the interaction lacks a quantitative measure

Clinical experience can help us with intensional qualitative problems of defining a class for study but we need quantitative methods to deal with the extensional problem of how many members are in the class. Without a measure of what is taking place in inclusive description, we lack a standard means of discussing and agreeing on our observations. Hence we come to the second question posed above.

Measurement

If some entity can be identified and observed in inclusive description, it can be measured since any defined quality can be assigned a quantity. To measure is to assign numbers or other symbols to observations. On a nominal scale, a symbol is used simply to classify an observation. We can then count the number of members belonging to a particular class. If observations classified on a nominal scale stand in some more-less relation to one another, we can rank them on an ordinal scale. Finally, there is the equal-interval scale of time which we can use to count frequencies and durations.

In evaluating observations of inclusive description in the analytic situation, we can state whether some quality is present, assigning it a classifying symbol, or absent, assigning it the symbol 0. Also we can rank order degrees of an observable quality. We can count how many time intervals contain a quality, how many qualities appear in a given time stretch, and how long a time stretch lasts containing a given quality. Therefore we can make a variety of quantitative measurements and with these measurements attempt answers to questions (2) and (3).

As mentioned, the clinical session does not provide an optimal situation to test hypotheses experimentally since it is designed primarily to carry out technological procedures, not to test and establish scientific generalizations. Even with methods of measurement to help us, there occur in clinical sessions too many uncontrolled interferences to conduct an ideal experiment. Too many questions are being considered simultaneously and there exist too many therapeutic responsibilities to manipulate variables. But the clinical analytic situation does represent a unique attempt at a controlled, artifically simplified, experimental human relation. It has the privacy, regularity, frequency, and constancy necessary for a prolonged systematic study of intimate and strong two-person interaction. It introduces variables not found in other human situations and it excludes variables usually present in real-life human relations while retaining their strong effects. If we could experimentally reproduce and refine the clinical situation, we might develop experimental ways of testing certain psychoanalytic hypotheses.

Experiment

An experiment consists of an interaction between factors under our control and a system we are studying. It is designed to answer a specific question and it is conducted in an artificially simplified situation in which some variables are controlled while others are isolated and deliberately manipulated to see if the manipulations produce predictable effects.

To experiment in a controlled way with the psychoanalytic situation is to enter unexplored territory. Initial trial and error attempts must be made to acquaint us with potential errors. We would like to experiment with controlled variations of the psychoanalytic situation to test clinical impressions and to obtain information about phenomena hidden beyond the range of clinical observation. For instance, question (3) above—how is the system or process predictably affected by the analyst?—seems especially amenable to experimental study. An analyst's verbal interventions, in particular, constitute deliberate attempts to effect a change in a process under observation. Such interventions represent selective input factors under our control and thus qualify as variables suitable for experimental manipulation.

To investigate inclusive description of free-association in the psychoanalytic situation, I have proposed a combination of clinical, quantitative, and experimental methods. But this is only an empty programmatic recommendation. Some begin-

ning attempts to fill it in empirically will now be described.

THE IMAGO SYSTEM

To listen to inclusive description of free-association in the analytic situation is to observe processes unfolding in time. The descriptions contain a great variety of elements. Which ones should we select to observe and to follow through time? It is a clinical impression that descriptions of personages constitute essential data in inclusive description of free-association. Let us examine this impression in a more detailed and precise way.

We can differentiate three phases in a temporal sequence of relations between a person and his personages. In the first phase, there exists some interchange between a person and his personage in the external world when they are in immediate proximity in space. In the second phase, events of these relations become represented or encoded structurally within the psychic apparatus of the person. I will term such intrapsychic representations of personages "imagos." In the third phase, portions of these imagos appear in the person's consciousness during free-association and are verbally described to the observer in the analytic situation. The analytic observer recognizes the presence of imagos by the names of personages appearing in inclusive description. I will term all the names used by the describer to identify personages as "personyms." A single imago will represent a conceptual unit of an inferred imago system made up of intrapsychic representations of personages. Thus an imago and an imago system are theoretical constructs while personyms are observables.

A summary of the definitions follows. *Observer*: a listener to inclusive description of a person's free-association in the analytic situation; *person*: describer of free-association; *personage*: a human being important and significant to the person and with whom the person has had a time-enduring, affectively-valued involvement; *personym*: an observable name used by the person to identify a personage; *imago*: a theoretical construct referring to a set of intrapsychic representations of a personage; *imago system*: a theoretical construct referring to the set of images assumed as a subsystem in the structural code of the psychic apparatus.

System, state and process

A system can be defined as an organized set of entities and their interrelations selected for study by an investigator. A state represents a condition of the system during some time interval when it is not undergoing change. A state can be described according to qualities, features, properties, or attributes which are termed "state variables," each variable having a range of values. A particular state of a system can be described by listing the state variables and their values for a specified interval of time. A process is defined as a sequence of states with the continuous parameter of time. To specify a process one specifies (a) what the system is, (b) what the states are, (c) what is the process law by which, from a state or sequence of states (process), other states or processes can be predicted. A process law represents an established explanatory generalization which when combined with a specific state or process of a system allows us to predict future states or processes of the system.

If a state or state-sequence of a system at a given time uniquely determines its state or process at another time, the system is considered to be state-determined. If state B invariably follows state A, the system is absolutely determined. If state B follows state A only in a regular percentage of instances, the system is probabilistic and Markovian. If no regularity of sequence between states or processes can be described, the system is completely indeterminate.

Changes, transitions, or alterations from one state or process to another can be of three kinds. If only one state variable changes, the transition will be termed "drift." If 2 or 3 state variables change, the transition will be termed "shift"; and if all state variables change, the transition will be termed "jump."

When a system is deliberately disturbed by the introduction of an input variable, a

transition may or may not occur. The probabilities of transition or no-transition can be estimated by counting the percentage of respective instances occurring with repeated experimental trials.

System chosen

I have chosen as a system for study the set of imagos referred to during inclusive description of free-association in the analytic situation. These descriptions provide observations which allow us to infer states of the imago system. A state of this system can be derived from observation of descriptions of personyms appearing during a specific time interval. A single imago will be taken as an inferred unit and a single personym as an observable unit.

Being arbitrarily selected, such an inferred system differs from a "real" system which embodies an infinite number of components. But selection of an imago system for study provides a useful simplification for three reasons. First, there exists a fortunately reliable one-to-one name relation between the name of a personage and the personym used in inclusive description. This allows us to avoid much of the ambiguity and redundancy of linguistic descriptions. Second, the number and variety of personyms appearing in inclusive description is small enough to provide a delimited and manageable area of investigation. And third, since an analytic observer becomes a component of imago systems, he represents an input factor which one can manipulate for controlled studies of these systems.

A state of the imago system is specified from observations of personyms named during a specific time interval of inclusive description. Units of observation can thus be related to units of time. After trying out various intervals, I have selected the 30-second interval as the most convenient because it is small enough to provide a large number of observations and large enough to contain one or more personyms. For example, let us take five minutes of tape-recorded inclusive description of free-association by a person and note the personages referred to during each 30-second interval starting at time 0 and dividing each minute in half. The written protocol would appear as follows:

Time	*Imago*
0 a)	Jean
b)	Jean
1 a)	Jean
b)	Mary
2 a)	Mary
b)	Mary
3 a)	Jean
b)	Jean
4 a)	Jean
b)	Jean

In this example, the analyst knows from what is being said that these personyms stand for significant personages, "Jean" being the person's wife and "Mary" being his sister. During this stretch of five minutes, the imago system can be assumed to have exhibited two processes: the state-sequences represented by "Jean," and the state-sequences represented by "Mary."

When we come to compare states or processes of this person's imago system with some other person's imago system, we meet the difficulties of comparing linguistic descriptions. Another person may not describe a "Jean" or a "Mary" and he may have no wife or sister. In this study, we are interested in individual persons only as members of the class of persons since we are searching for dependable class generalizations. Hence we must find a way of categorizing imagos so that generalizations can be formulated.

Categories of measurement

Each imago referred to can be assigned to categories and the categories represented by symbols. I have chosen four categories to represent state variables of the imago system. Each imago referred to in inclusive description can be classified and given values in these four categories. They are as follows:

Category (1): Coupling. The coupling refers to a time-enduring involvement between person and his personage, representations of which are mentioned or alluded to whenever a personym is used. Three ordinal degrees of intensity of in-

volvement will be used. (a) The capital letter *T* indicates a tight coupling; (b) the capital letter *C* indicates a close coupling; (c) the capital letter *L* indicates a loose coupling.

Category (2): Affective value. During inclusive description, a person attaches a variety of affective values to the personyms described. The values are grouped as positive (like, approve, desire, praise, admire, enjoy, etc.); negative (dislike, hate, fear, scorn, anger, etc.); and null, i.e., when no affective value is present or when the affective value cannot be considered either positive or negative. Two degrees of positive and negative values will be ranked ordinally. (a) The small letter *p* indicates a positive value, and *pp* a strongly positive value; (b) the small letter *n* indicates a negative value, and *nn* a strongly negative value; (c) the small letter *o* indicates a null value.

Category (3): Developmental rank and life phase. This represents a double category specifying not only a certain type of personage but the phase in the person's life when this personage is being referred to during inclusive description. Three ranks and three phases are differentiated. The ranks are: (a) the number 1 stands for a primary imago representing a father or mother; (b) the number 2 stands for a secondary imago representing a sibling or relative; (c) the number 3 stands for a tertiary imago representing a girl friend, boy friend, wife, husband, etc. The phases are: (a) the small letter *c* stands for childhood up to the age of puberty; (b) the small letter *y* stands for youth from puberty to adulthood; (c) the small letter *a* stands for adulthood.

Category (4): Sex. This category is concerned with the actual biological sex of the personage. (a) The capital letter *M* indicates a male; (b) the capital letter *F* indicates a female.

At first glance, one might wonder why these particular categories were chosen. A classification is selected according to what one wants to use it for. Its use should become clearer in the next section.

Questions regarding the suitability of these choices to describe person relations may also be raised. The categories are limited in scope and do not capture the richnesses of relations between a person and his personages. But in exploratory investigations one must start simply even if starkly.

More important would be questions as to the reliability of these choices. Categories (2), (3), and (4) require only direct observation while category (1), Coupling, involves a degree of inference. It might be difficult to achieve agreement among multiple independent observers as to what is a tight versus a close coupling. When the relation is that of wife to husband or man to love-object, most would rank these as tight couplings. But other relations between friends or between adult son and parent may be difficult to rank. Hence the rank orders of category (1) may be of modest reliability.

Having selected units of observation and categories for measurement, let us now consider a process having a continuous parameter of time.

The imago process and its score

A column vector containing five symbols will be used to describe notationally a single state of the imago system. For example, the vector

$$\begin{bmatrix} \text{T} \\ \text{p} \\ \text{3a} \\ \text{F} \end{bmatrix}$$

would indicate an imago representing a tight coupling, positively valued, with a tertiary, adult, and female personage.

A sequence of column vectors will be used to describe a sequence of states. They can be written out like a muscial score on blank music score sheets. For example, let us consider a stretch of 30 minutes of inclusive description. The written protocol appears as follows, dividing each minute in half and recording those personyms appearing in that time interval.

Written Protocol

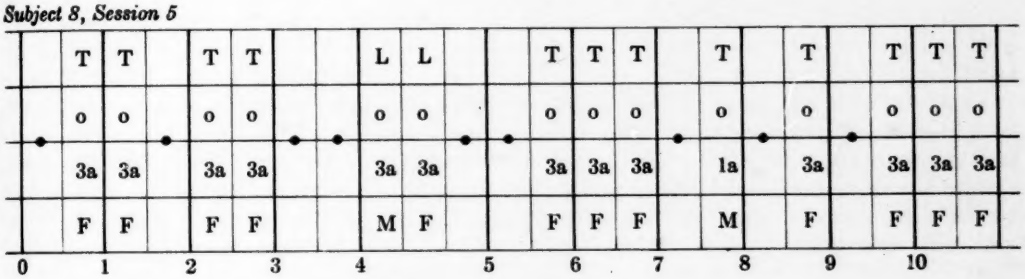
Subject 8, Session 5

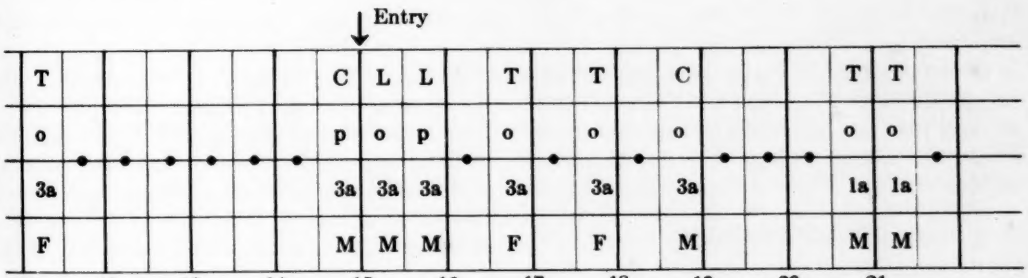
0 a) none
  b) Jean T o 3a F
1 a) Jean T o 3a F
  b) none
2 a) Jean T o 3a F
  b) Jean T o 3a F

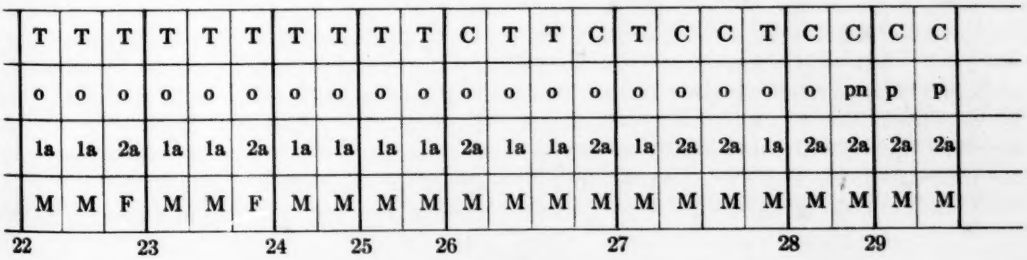
3 a) none
b) none
4 a) male friend L o 3a M—
female friend L o 3a F
b) none
5 a) none
b) Jean T o 3a F
6 a) Jean T o 3a F
b) Jean T o 3a F
7 a) none
b) father T o 1a M
8 a) none
b) Jean T o 3a F
9 a) none
b) Jean T o 3a F
10 a) Jean T o 3a F
b) Jean T o 3a F
11 a) Jean T o 3a F
b) none
12 a) none
b) none
13 a) none
b) none
14 a) none
b) close male friend C p 3a M
15 a) analyst L o 3a M
b) analyst L p 3a M
16 a) none
b) Jean T o 3a F
17 a) none
b) Jean T o 3a F
18 a) none
b) close male friend C o 3a M
19 a) none
b) none
20 a) none
b) father T o 1a M
21 a) father T o 1a M
b) none
22 a) father T o 1a M
b) father T o 1a M—sister T o 2a F
23 a) father T o 1a M
b) father T o 1a M—sister T o 2a F
24 a) father T o la M
b) father T o 1a M
25 a) father T o 1a M
b) father T o 1a M
26 a) uncle C o 2a M—father T o 1a M
b) father T o 1a M—uncle C o 2a M
27 a) father T o 1a M—uncle C o 2a M
b) uncle C o 2a M—father T o 1a M
28 a) uncle C o 2a M
b) uncle C pn 2a M
29 a) uncle C p 2a M
b) uncle C p 2a M

The score of the state-sequences or imago process would appear as:

Subject 8, Session 5






T T T T T T T T T T C T T C T C C T C C C C
o o o o o o o o o o o o o o o o o o o pn p p
1a 1a 2a 1a 1a 2a 1a 1a 1a 1a 2a 1a 1a 2a 1a 2a 2a 1a 2a 2a 2a 2a
M M F M M F M M M M M M M M M M M M M M M M
22 23 24 25 26 27 28 29

It will be noted that some 30-second intervals contain more than one personym and hence more than one imago state. Also, there are intervals in which no personym appears. Many of these contain the self as an object of description but by definition an imago is a representation of a significant *other* person.

To study a single state in the process we can examine the contents of a single column vector. To follow a particular state variable through the process we examine the contents of horizontal row vectors. Thus the imago process score can be read both vertically and horizontally and counts made of columns and rows.

In this particular example, it can be seen that during the second 15 minutes the imago system increased in activity, and a drift in the imago process occurred in rows 3 and 4 from tertiary to primary and secondary, and from male and female to preponderantly male. These sorts of changes will be considered more thoroughly in the Discussion.

This completes a brief outline of the system-state-process selected for observation and the notation that will be used. Such a system-state-process model has advantages at the observational level not available to kinematic geometric models (Colby, 1955). The imago system-state-process model allows us a closer tie between theoretical construct and observable, since observation in the analytic situation concerns identifiable processes linear in time. In the next section, we will take up attempts to experimentally affect the imago system and examine the results.

THE EXPERIMENT

Question (3) above asked, "How does an analyst predictably affect a process under observation?" Having selected state-sequences of the imago system as important and measurable aspects of inclusive description of free-association, we should be able to test some effects of an analyst on this process since his presence and interventions represent isolable factors suitable for experimental manipulation. The working hypothesis for this experiment asserted that the physical presence of an observer activates the imago system. The hypothesis originated as a hunch from clinical experience.

Method

An attempt was made to reproduce a controlled variation of the analytic situation. The physical setting for the experiment was my private psychoanalytic office used for the clinical treatment of patients. The only difference in the experimental setting was the presence of a tape recorder behind the couch.

To obtain subjects (experimental "persons"), a notice was posted on the bulletin board of a medical school stating that volunteers would be paid $5.00 per hour to participate in psychological experiments on free-association. I interviewed each applicant briefly and gave him the following explanation:

"This is an experiment on free-association. The idea is that I will pay you $5.00 an hour to come in each evening four times a week for three weeks. You will lie on that couch and for half an hour describe everything that comes into your mind. What you say will be recorded on this tape recorder. I guarantee you complete anonymity. The tapes will be listened to only by myself and a technician, and no names or other possible identifying data will appear in the write-up of the experiment."

Out of 16 people who volunteered, 11 could meet the demanding time schedule. The experimental group was fairly homogenous, consisting of ten medical students and one resident. They were all males and ranged in age from 23 to 30. None showed severe psychopathology and none had been patients in psychotherapy. Each subject served as a self-matched control in a "before-after" type of experiment. The sessions were designed as follows.

At the start of the first session, the orienting instructions stated above were repeated with an emphasis on excluding nothing and describing everything. During the first week, each subject lay on the couch and described aloud the "free-associative" contents of self-observation. He was alone in the

office with a tape recorder behind the couch. Each session lasted one-half hour. Communication between subject and myself was limited to friendly greetings and goodbyes. These first four sessions were designed to acclimate the subject to the setting and to establish a baseline for comparison.

During the second week, all conditions were kept the same except that exactly 15 minutes after the start of the session, I entered the room and sat as an observer in a chair behind the couch with the tape recorder still running. Upon the first entry I explained, "Now today I will be in the room with you part of the time. However, the conditions of the experiment require that I not say anything. I will just sit here and take a few notes. You go on describing everything that comes into your mind." I was careful to say absolutely nothing during the second half of the session while I was present. The notes I made consisted of protocols described above and other general observations regarding the subject's behavior. In the three remaining sessions of this week, upon entry after 15 minutes, I simply said, "The same thing as yesterday."

During the third week, there were two sessions in which I again did not enter the office at all as in the first week. Three subjects were interviewed in their final session to explore their thoughts and feelings about the experiment.

Results

General observations. Some general but pertinent observations might first be made. All of the subjects reported in their inclusive description a subjectively appreciable rise in tension and feeling of inhibition when the observer entered and remained during the second half of the session. However, this was not objectively observable in measuring the rate of their speech except in Subject 3. Subject 3 described marked anxiety with palm sweating during the analyst's presence and reduction of his rate of speech was noticeable. The rise in tension with the analyst present was described by the subjects as decreasing after five or ten minutes and as the sessions of the second week progressed, it subsided almost entirely. Two subjects described their inclusive description as being both harder and easier with the analyst present. It was harder because of the slight anxiety aroused but easier because there was another person immediately present to hear the communications rather than a machine with its eventual listener far removed in space and time.

All of the subjects wondered what the specific purpose of the experiment was. During inclusive description when the analyst was not present, they made such statements as, "I wonder what he's looking for?", "What will he make out of all this?", "Is he making a study of medical students?" Since they experienced slight anxiety and dampening with the analyst's first entry and presence, all of them concluded the experiment was designed to study the inhibitory effect of another's presence on free-association. In view of this conclusion, it is interesting that the results of the experiment showed the observer's presence to have actually an opposite activating effect which went unnoticed by the subjects.

During the third week, all the subjects began to lose interest in the work. They reported a growing boredom with listening to themselves free-associate. Some began to arrive late, some described the work as a drag, and some even fell asleep for a minute or two during the session. It would seem that it becomes increasingly difficult to describe free-association out loud, hour after hour, without receiving some acknowledging verbal response from a listener.

In their final sessions, Subjects 8, 9, and 11 were interviewed in order to ask them specific questions about their impressions of the experiment. They repeated their ideas and feelings as reported during the sessions. To the question "What do you think the chief problems were that you talked about?", they all mentioned schoolwork since it comprised most of their life at the moment. As for censorship, all three stated they occasionally left out of their descriptions a sexual thought, although on the tapes there is no dearth of sexual information. When asked directly as to the specific aim of the experiment, none of the

subjects in their replies guessed it. Subject 8 thought I was making a study of the way medical students free-associated in order to compare them with other people. Subjects 9 and 11 replied that I was studying the fact that the presence of another person in the room inhibits free-association.

Subject 1 was not used during the third week for this particular experiment but was a participant in another experiment. However, the sessions of his first two weeks were identical with the other subjects. A few times the tape recorder failed to record properly but since this was unknown to the subject and the analyst the session was run off nevertheless. Subject 10 contracted an infectious illness during the second week and could not complete the experiment.

Quantitative data. Quantitative estimates were made to evaluate the effect of the analyst's presence on imago systems of the subjects.

The first quantitative measure consisted of a count of the number of 30-second time intervals in which one or more personyms appeared. Since each session lasted 30 minutes, there were 60 possible intervals. Tables 1, 2, and 3 give the *number of intervals* in which one or more personyms appeared during the sessions. Subjects 10 and 11 did not appear for Session 4 of the first week because of (separate) accidents.

Totalling each subject's score for the first

TABLE 1

FIRST WEEK

NUMBER OF 30-SECOND INTERVALS CONTAINING ONE OR MORE PERSONYMS DURING 30-MINUTE SESSIONS DIVIDED INTO TWO 15-MINUTE HALVES

The observer was not present during the sessions.

Subjects	Session 1		Session 2		Session 3		Session 4	
	1st	2nd	1st	2nd	1st	2nd	1st	2nd
1	13	20	26	26	20	20	18	8
2	*	*	10	9	10	14	11	11
3	10	14	10	7	17	11	8	13
4	3	5	6	7	9	19	7	6
5	*	*	15	12	16	18	24	17
6	21	22	24	27	27	30	26	24
7	26	22	13	19	27	28	22	18
8	8	9	7	5	12	10	6	11
9	15	5	19	5	7	10	*	*
10	*	*	23	19	24	23	†	†
11	25	15	19	12	20	24	†	†

* Session misrecorded.
† No session.

TABLE 2

SECOND WEEK

NUMBER OF 30-SECOND INTERVALS CONTAINING ONE OR MORE PERSONYMS DURING 30-MINUTE SESSIONS DIVIDED INTO TWO 15-MINUTE HALVES

The observer was present during the second half of each session.

Subjects	Session 1		Session 2		Session 3		Session 4	
	1st	2nd	1st	2nd	1st	2nd	1st	2nd
1	12	18	11	21	13	17	20	22
2	14	21	13	15	12	18	9	11
3	8	6	10	11	9	15	6	12
4	4	12	12	13	20	10	16	9
5	22	24	14	16	17	17	13	26
6	27	30	25	28	19	29	11	11
7	15	23	5	20	18	29	23	16
8	12	13	17	17	14	15	17	17
9	20	25	15	23	22	19	24	30
10	21	28	24	25	†	†	†	†
11	22	18	25	20	15	27	18	24

† No session.

TABLE 3

THIRD WEEK

NUMBER OF 30-SECOND INTERVALS CONTAINING ONE OR MORE PERSONYMS DURING 30-MINUTE SESSIONS DIVIDED INTO TWO 15-MINUTE HALVES

The observer was not present during the sessions.

Subjects	Session 1		Session 2	
	1st	2nd	1st	2nd
1	†	†	†	†
2	7	7	11	2
3	14	7	16	12
4	11	10	10	17
5	19	7	14	14
6	14	12	20	18
7	23	23	25	22
8	9	14	*	*
9	9	13	8	8
10	†	†	†	†
11	15	19	21	10

* Session misrecorded.
† No session.

half and comparing it with his total for the second half, the increments found during the observer's presence in the second week are statistically significant by the t test (p = .001) and Wilcoxon's Matched-Pairs Signed Ranks Test (p = .002). Since intervals containing personyms of the observer were counted, it might be thought that the rise was due solely to an increased mention of the now-present observer. However, if all mentions of the observer are subtracted from both halves of the session, the increment during the second half is still significant at the .004 level. During the first and third weeks when the observer was not present, there is no significant difference between the first and second halves of the sessions.

An increase in personyms referring to the analytic observer occurred at a statistically significant level (.01) only in the first session of the second week. No significant progressive effects were observable during the second week.

From these statistical estimates it would appear that the physical presence of an observer served as an input which activated the imago system, stimulating it to a quantitative increase in output.

Were any qualitative changes observable? From the process score, i.e., the state-sequences of the imago system, it was noted that during the observer's presence a statistically significant change took place in the state variable representing the sex of the imago. Tables 4, 5, and 6 give the number of male and female *imago states* occurring in the first and second halves of each session. Summing the total males and total females for each subject (omitting mention of the observer) and taking the difference between these differences, an increment in males took place in the second half of the sessions significant at the .05 level using the Wilcoxon Test. No such increment occurred in the first or third weeks when the observer was absent.

None of the other three state variables showed a process change of any regularity or statistical significance. Hence it would seem that the presence of the observer not only raised the total activity of the imago system but also produced a process change in the form of a drift towards a preponderance of male imagos.

Possible sources of error

Before attempting conclusions and interpretations of the data, several possible sources of error in the experiment should be considered.

TABLE 4

FIRST WEEK

NUMBER OF MALE AND FEMALE IMAGO STATES DURING 30-MINUTE SESSIONS DIVIDED INTO TWO 15-MINUTE HALVES

The first of the hyphenated numbers represents males and the second females. The observer was not present during the sessions.

Subjects	Session 1		Session 2		Session 3		Session 4	
	1st	2nd	1st	2nd	1st	2nd	1st	2nd
1	1-15	8-16	7-23	4-27	5-12	16-7	12-6	3-5
2	*	*	1-8	6-6	9-3	12-9	7-4	9-6
3	4-7	10-5	5-7	3-5	18-5	3-15	3-6	13-8
4	4-5	6-2	8-2	5-4	9-1	18-9	5-0	5-4
5	*	*	9-12	12-4	13-11	14-7	19-15	6-17
6	17-8	15-14	26-21	19-25	34-21	31-33	28-28	26-14
7	14-27	13-22	4-12	16-11	26-11	25-25	4-25	6-14
8	10-3	9-5	9-2	9-4	13-5	10-1	8-1	11-2
9	13-7	3-3	14-7	3-3	5-5	5-5	*	*
10	*	*	18-14	15-13	21-11	18-21	†	†
11	33-7	8-7	12-9	11-3	18-3	35-1	†	†

* Session misrecorded.

† No session.

TABLE 5

SECOND WEEK

NUMBER OF MALE AND FEMALE IMAGO STATES DURING 30-MINUTE SESSIONS DIVIDED INTO TWO 15-MINUTE HALVES

The first of the hyphenated numbers represents males and the second females. The observer was present during the sessions.

Subjects	Session 1		Session 2		Session 3		Session 4	
	1st	2nd	1st	2nd	1st	2nd	1st	2nd
1	10-2	15-6	5-8	17-7	6-10	8-10	7-15	18-11
2	12-5	20-4	12-4	11-5	16-2	15-7	8-1	9-4
3	6-4	5-2	9-4	5-10	6-3	10-8	3-3	6-6
4	4-1	13-0	12-1	13-2	17-12	10-0	16-4	10-0
5	9-21	20-6	12-19	14-2	9-10	14-6	11-6	8-23
6	21-13	29-8	20-15	29-11	14-14	30-1	6-5	8-5
7	3-13	23-4	6-0	19-2	20-3	18-46	9-19	9-10
8	11-3	10-3	16-5	16-3	13-4	6-11	12-8	16-7
9	11-13	11-25	12-3	25-2	13-6	19-3	18-10	30-0
10	10-14	17-18	12-20	16-19	†	†	†	†
11	21-9	17-6	13-12	28-8	12-16	20-6	20-7	18-11

† No session.

In an experiment of this kind, the most likely source of error would lie in the relation between observer and subject. Since the observer was a common denominator in all instances, one might suspect an observable regularity to stem from some constant underlying error on his part. By his behavior he might have indicated to the subjects what sort of changes he expected to find, and in order to please him, they might produce these changes.

I attempted to keep the relation between myself and the subject at minimum intensity by maintaining a benevolent but businesslike reserve, indicating a disinclination to communicate except for the routine greetings and farewells. Under these cautious conditions, it is unlikely that I indicated by my own behavior what specific effect was being or should be elicited by my presence during the second half of the session.

Conscious and unconscious motives of the subjects might be considered as effecting the particular changes that occurred in the experiment. All of the subjects were working or had worked at low-paying jobs to supplement their income. The $5.00 per hour represented an attractive wage to them for what they considered very easy work. They all described this opinion at some time or other during their inclusive descriptions. Some, of course, felt the process to be mildly therapeutic in the cathartic sense but none were consciously seeking it for that purpose. Their unconscious motives, of course, would require a great deal more information to make reliable inferences. My feeling would be they were not uniform enough to produce the regularities found. One subject stated

TABLE 6

THIRD WEEK

NUMBER OF MALE AND FEMALE IMAGO STATES DURING 30-MINUTE SESSIONS DIVIDED INTO TWO 15-MINUTE HALVES

The first of the hyphenated numbers represents males and the second females. The observer was not present during the sessions.

Subjects	Session 1		Session 2	
	1st	2nd	1st	2nd
1	†	†	†	†
2	6-3	5-2	12-7	1-0
3	11-3	4-2	14-4	12-4
4	13-0	3-7	7-5	14-8
5	10-16	3-4	11-3	6-8
6	13-6	8-5	18-12	8-18
7	21-11	12-17	25-15	14-10
8	7-2	14-8	9-3	10-5
9	9-1	10-6	8-2	7-3
10	†	†	†	†
11	6-9	17-7	11-11	8-4

† No session.

that some day he would like to be analyzed. A third conscious motive was verbalized by some of the subjects, e.g., to contribute to science, to share in the discovery of knowledge. All of these motives combined to facilitate cooperation with the analytic observer but they could not account for the specific quantitative changes found. The subjects willingly accepted the orienting experimental instructions and attempted to give the analyst what he requested—inclusive descriptions of their self-observation. But since none of them sensed his particular focus of interest, it is doubtful that motives for participating in the experiment, conscious or unconscious, could have produced the specific results observed.

It might be wondered whether these particular subjects, medical students and one resident, might anticipate what sort of effects the observer would like to see from his presence because of their educational background and current work. Naturally, a psychiatrist and psychoanalyst has particular significance for medically trained persons. To them, he is a professional specialist experienced in understanding free-association and knowing what people "mean" by what they say. Also, he is known to be interested in childhoods, sexual matters, and unusual thoughts. Yet none of these topics increased during the observer's presence. The imago system and process, being described for the first time in this paper, is not a part of medical or psychoanalytic knowledge which might be reproduced by the subjects to please the observer.

Three of the subjects in the experiment were friends and at times they discussed their experiences in free-associating. Because of this, two subjects knew ahead of time that the observer would be present during the second week. But they anticipated only that they might feel more tense in his presence. One of the subjects attended a class in psychiatric diagnosis which I had conducted two years previously. None of the others had met me before.

Other errors might conceivably have originated from situational factors. However, the physical setting was the same for the three weeks and exactly the same before and after entry. Outside noises, such as rain, passing trucks, overhead furniture-moving, people yelling in the street, etc., were too random and infrequent to correlate with the regular changes found. Variations in administering the experiment were not a factor since all subjects were treated in the relevant aspects as alike as can be achieved in human situations.

Personal factors such as physical illness, transient psychological problems, or particular life situations can be ruled out as accounting for the effects. Errors in counting or computation were reduced to a minimum by repeated countings both by myself and another person.

Finally, before interpreting the data, it should be mentioned that the size of the sample and the number of sessions are sufficient to make inferences which have statistical significance.

In sum, the possible constant and random errors discussed did not play a significant part in the results of the experiment. The changes found in the activity of the imago system and in the imago process were related to the input of the analyst's presence and not to solely internal or other external factors.

DISCUSSION

The working hypothesis—that the physical presence of an observer alone is sufficient to activate the imago system in some regularly discernible way—tends to be confirmed by the experimental findings. The effects observed might have been more sharply demonstrated if during the first week the subjects felt that absolutely no observations were being made on their free-association. They knew the analyst would eventually listen to the tapes so in one sense there was an analyst present, as represented by the tape recorder, during the first week and during the first half of sessions of the second week. In order to compare differences between observed and absolutely unobserved inclusive description of free-association (as far as the person is concerned), one would have to deceive subjects by telling them their verbal descriptions were not being recorded during certain periods. I

doubt if the yield would be worth the lie. Subjects are not dolts and they would quickly conclude there would be no need to free-associate out loud when alone unless it were being recorded.

The changes produced by the analyst's presence did not occur 100% of the time. They occurred only in a high percentage of the experimental trials. This would indicate that the imago system should not be thought of as strictly determinate in any absolute way but probabilistic, i.e., determinate in a regular percentage of instances. The output of the imago system represents a combination of input and internal variables. If a change in output can be correlated with a specific input in a regular percentage of instances, we are justified in assuming that the system is probabilistic in a statistico-determined sense.

On the basis of clinical knowledge, one might expect other changes to have been produced. A progressive effect of the over-all experiment might have been anticipated, as for example a drift of the couplings with the observer from loose to tight or a gradual increase in references to the analyst. However, these were not observed. The many other things not found are bounded only by one's imagination. It is usually a waste of time to speculate about negative findings. Let us consider only the positive results and their interpretations.

The experimental results indicate that during periods of an observer's silent presence (a) the imago system increased in activity, and (b) the imago process drifted towards a male preponderance, as compared with periods when he was not present. I would generalize from these facts that in the *analytic* situation the physical presence of an analyst is alone sufficient to induce (1) quantitative increments in imago system activity, and (2) a qualitative process drift in the sex state-variable. In higher-level theoretical terms, it could be stated that the entire class of structural representations of personages becomes hypercathected by the immediate presence of a particular member of this class in the external world. Further, a subdivision of this class, i.e., structural representations of males, may become selectively hypercathected by the immediate presence of a male personage in the external world. Neither of these effects are consciously realized to be taking place by persons thus affected.

These conclusions can be seen to have implications for clinical psychoanalytic treatment and perhaps even for everyday human relations. It may be unjustified to extend generalizations from this experiment to everyday human relations, not so much because the experimental group was unrepresentative of a hypothetical everyday population, but because the contrived experimental relation between subject and observer was so distortingly artificial. The clinical relation between patient and analyst, while to some extent artificial, remains representative in many aspects relevant to everyday relations. But the contrived experimental relation described here represents an extreme distortion, doing violence to relations which hold in everyday life.

But accepting the usual inductive risk and, in this instance, its harmless penalties, one might at least wonder whether in our everyday human relations the immediate presence of a given personage mobilizes in us messages about a variety of our other personages, and particularly those of the same sex as the one immediately present. Of course, when coupled persons are in immediate proximity, they usually speak to one another and the effects of this communication on imago systems may far overshadow the effects of simple approximation in time and space. But even this we cannot as yet assert with confidence.

The conclusions are much more relevant to the clinical psychoanalytic situation. Although a patient's motive to seek help and benefit, and an analyst's motive to offer help and benefit, were not present in the experimental situation, there was sufficient similarity to justify extending generalizations from one to the other. Also, the experiment lasted over a very short time-stretch compared to the duration of psychoanalytic treatment. Over a longer stretch, the experimental effects observed might wear

off or be interfered with by the verbal interventions of the analyst. The clinical and experimental situations have similarities and dissimilarities but, again, the similarities seem sufficiently strong to risk analogies.

In the clinical psychoanalytic situation, we observe certain regularities. It was mentioned earlier that a person's descriptions of his personages appear clinically to be important ingredients in free-association. Conclusions from the experimental findings imply that an analyst hears a lot about the class of the patient's personages, not only because personages are important, but also because the analyst is immediately present as a member of that class. The analyst's physical presence introduces a type of parallax into his observations, because that presence alone has specific effects on the observed patient. I am not suggesting that this parallax represents a serious bias in psychoanalytic observation. Even if the sex of the analyst induces a process drift, it is not a change resulting in the exclusion of the other sex. Also, as in everyday relations, the effects of an analyst's verbal interventions may outweigh the effects of his silent presence in the analytic situation. However, the experiment uncovered two phenomena hidden from the range of our usual clinical observation, since an analytic observer is inevitably present in the clinical situation. The effects discovered may be thought to have some quantitative and qualitative influence on observation in the psychoanalytic situation, but they are of perhaps greater relevance for the aims of psychoanalytic treatment.

One aim of psychoanalytic treatment is to increase a patient's awareness of his own intrapsychic contents and operations. Clinically, we know that this can be achieved through collaborative efforts between patient and analyst over long periods of time. It is believed that an analyst aids an increase in awareness through appropriate procedures and interventions. Conclusions from this experiment imply that an analyst facilitates this increase in a specific way and to a significant degree *just through the fact of his being present with the patient.* The immediate physical presence of an analyst can activate imago systems, bringing more imagos and their relations into a patient's conscious awareness without his conscious realization that this effect is taking place in his free-association.

Before patterns of a patient's behavior can be subjected to his rational consideration and to interpretation by an analyst, they must first be mobilized close to or into the patient's conscious awareness. Mobilization by itself is of course insufficient for a reorganization of these patterns but it constitutes the necessary initial step.

The second conclusion, that an analyst's presence can induce a specific qualitative process change in the direction of the sex of the analyst, would have importance for the relation between patient and analyst as the patient views it, and for his transference reactions. Also, a male analyst may simply hear more about males from a male patient because the analyst is male. However, clinical implications of the second conclusion should be viewed with caution. The experiment was conducted by a male analytic observer with all male subjects. While I would be willing to extend the first conclusion to patients of both sexes, since the biological sex of the participants does not seem to be relevant, I would not think the experimental results yet justify a conclusion that the sex state-variable process drift is towards the sex of the analyst. It might be that in order for such a process change to occur, the sex of the person must correspond to the sex of the analyst. While the experimental finding, that an observer's presence can produce a process change towards males, certainly constitutes a suggestive fact, the conclusion that it is due solely to the sex of the observer remains to be explored. Other experiments should be conducted between a male observer and female subjects, and between a female observer and male subjects, to find out whether the effect is determined by the sex of the observer alone or in conjunction with the corresponding sex of the subject.

It is obvious that persons affect other persons in everyday life, and particularly in

the clinical psychoanalytic situation. But we want to know more about the precise nature of these effects and about the specific factors responsible. If we can isolate and refine these factors, we may be able to increase our control over them. This has been an experimental contribution towards that end.

REFERENCE

Colby, K. M. *Energy and structure in psychoanalysis.* New York: Ronald Press, 1955.

(Manuscript received March 18, 1959.)

Popular resistance has been especially strong against religious reformers and men of science. That saints and scientists should thus be placed in a single category by the opposition of others is far more than a casual occurrence. They have much in common, above all, their other-worldliness. There are but few saints among scientists, as among other men, but truth itself is a goal comparable to sanctity. As the Pythagoreans had already understood it more than twenty-four centuries ago, there is sanctity in pure knowledge, as there is in pure beauty, and the disinterested quest of truth is perhaps the greatest purification.

However, the hostility to saints and to investigators was not altogether of the same kind. Attempts at religious reform aroused popular anger because the inborn conservativeness of man is nowhere stronger than in the field of religion. The religion of his fathers must not be criticized, even if his own profession of it is but an outward show. The most malicious kind of hatred is that which is built upon a theological foundation. On the other hand the resistance to scientific novelties was due to an intuitive, if unconscious, appreciation of their revolutionary nature. The slightest and the most innocent scientific innovation is but a wedge which is bound to penetrate deeper and deeper and the advance of which will soon be impossible to resist. Conservative people are undoubtedly right in their distrust and hatred of science, for the scientific spirit is the very spirit of innovation and adventure—the most reckless kind of adventure into the unknown. And such is its aggressive strength that its revolutionary activity can neither be restrained nor restricted within its own field. Sooner or later it will go out to conquer other fields and to throw floods of light into all the dark places where superstition and injustice are still rampant. The scientific spirit is the greatest force for construction but also for destruction.

GEORGE SARTON, *The History of Science and the New Humanism*

The current structure of real-life operating organizations is in large part an outcome of their problem-solving histories. The experiment reported here relates organizational development and performance in laboratory groups to variations in the historical sequence of the tasks they have worked on, in an attempt to explore what effect the order of the given task has on the developing structure of the organization: how solutions to problems emerge and how "roles" are assigned within an organization.

TASK ORDERING AND ORGANIZATIONAL DEVELOPMENT IN THE COMMON TARGET GAME[1]

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INTRODUCTION

THE experiment described here is one of a series of exploitations of a group situation we have labelled the "Common Target Game."[2]

The "Common Target Game" can be played as follows: (1) Four *S*s (or more, or fewer) are prevented from communicating directly with one another. (2) Each is told that he may contribute to a kitty any of the eleven integers from 0 through 10 by holding up fingers, or by other signals, directly and solely to *E*. (3) *E* calls out a target number between 0 and 10 times the number of *S*s. (4) The object of the game is that members learn to contribute together precisely the target integer called for, on their first effort.

Thus, for a four-man group, *E* may call out as a target any whole number between 0 and 40. Each *S* must independently select some whole number from 0 through 10 as his contribution to the target and communicate it only to *E*. *E* sums the four contributions.

In the current version of the game, *E* then feeds back to the total group the following information: (1) the actual sum of the contributions of the four *S*s ("Your target was 23, you actually hit 27."); (2) the distribution of contributions, not individually identified, beginning with the smallest contribution and proceeding to the largest ("You actually hit 27 with one 5, two 7's and an 8."). In this version of the game, too, *E* repeats the target until the group makes a direct hit. After a hit, *E* calls another target. In their preliminary instructions, *S*s are told that their purpose is to try to develop a "system such that after several trials you will be able to hit any target in one effort."

In this experiment a particular target series was used with one-half of our experimental groups, while the other half received the same series, but in reverse order.

SOME CONCEPTS

It is necessary to digress at this point into some analysis of possible solutions to the Common Target Game and into possible behaviors of *S*s. Several points need to be made: (1) A variety of programs can be constructed, each of which will be satis-

[1] My thanks to Professor Warren Bennis of Boston University for help in this study. We worked on several phases of the general area together at the 1957 Social Science Research Council Seminar on Organization Theory at Carnegie Institute of Technology. My gratitude, too, to Messrs. Bernard Weiner and Robert Groh of the School of Business, University of Chicago (where this experiment was conducted), for their help in organizing data and running subjects; and to the University of Chicago's School of Business research fund. Professors Jacob Marschak and Richard Willis provided most helpful criticisms of the manuscript.

[2] The "common target" game has its origin, to the best of my knowledge, in the same place as the networks idea; to wit, in Dr. Alex Bavelas. He was trying out variations of it in 1948.

factory for solving any target in one effort. It is not clear that these alternatives can be definitively ranked on any scale of "goodness" or rationality. (2) Several general programs can be constructed which can solve *almost* all targets in one effort. It is possible, but not easily demonstrable, that some of these imperfect solutions may be easier to implement than type (1) solutions when communication among players is forbidden. (3) Some general programs exist which can solve any target in *two* efforts; or in other specific numbers of efforts larger than one. (4) It is possible analytically to differentiate out at least three large substeps in the process of devising a general program for solving the game: (a) the determination of a set of target *categories*; (b) the determination of a *system of roles*, utilizing the set of target categories; and (c) the *assignment of those roles* to particular individuals. A brief consideration of each of these steps is in order.

Categories

Any general scheme for solving any target in one effort requires some classification of all possible targets into some set of categories, each of which is smaller than the universe of targets; otherwise, of course, the solution to each target would need to be independently worked out and memorized. Several categorizations are possible. A 4-man group may divide all targets into a set of four categories such as these: (a) targets evenly divisible by 4 ($4x$); (b) targets divisible by 4 with a residual of 1 ($4x + 1$); (c) those with a residual of 2 ($4x + 2$); and (d) those with a residual of 3 ($4x + 3$). As a subset of alternatives, one can classify targets from the next *lower* integer divisible by 4; so that residuals are counted upward (e.g., the target 25 is seen as $24 + 1$, or of the class $4x + 1$); or so that residuals are counted downward (25 now is seen as $28 - 3$, or of the class, $4x - 3$); or they may use the nearest base (e.g., $25 = 24 + 1$, but $27 = 28 - 1$.)

Note that in our experimental case, E has two imperfect ways of detecting S's categorization: S's protocols, and/or the fit between E's alternatives and S's performance over several targets.

But other prominent possible categorizations exist, including sets based upon division of targets by 10 or by 3.

Role systems

The categories selected by a group constrain but do not uniquely determine a set of organizational roles that will have to be performed. Thus if a group had decided on a $4x + r$ set of categories, it might proceed to select any one of several systems of organizational roles. Differences among role systems become especially apparent for target numbers falling in the class $4x + 3$.

Here, for example, are three different role systems that might be used on a target number 27, all within the category system $4x + r$.

Target Number = 27.

System (1):

Member	A	B	C	D
Contributes	6	6	6	9

System (2):

Member	A	B	C	D
Contributes	6	6	7	8

System (3):

Member	A	B	C	D
Contributes	6	7	7	7

Note that (1) and (2) will not work for all targets, e.g., 39. We call (1) a concentrated role system, (2) a semi-concentrated, and (3) a non-concentrated role system.

Note that more than one role system is possible for other categories than $4x + r$. For example, categorization of all targets by 10's also allows for more or less concentrated role systems. Note, too, the behavior of a group on a single target does not uniquely define the categories or role system the group may be using. Thus the behavior shown in (3) above,—6, 7, 7, 7—*could* be the outcome of a $4x + r$ categorization coupled with a deconcentrated role system. But it could as well result from a $4x - r$ categorization coupled with any of the three role systems just described.

Role assignments

Given a group that has now settled upon a set of categories and a role system, there remains the task of allocating particular roles to particular group members; or at

least devising a program of allocation, so that when a target is at hand, all roles are performed and none is duplicated.

TASK ORDERING

The current structure of real-life operating organizations is probably in large part an outcome of their problem-solving histories. Given different task histories (e.g., different products, different economic environments, etc.) at different stages of their growth, we would expect different present operating organizational structures (e.g., different distributions of line and staff roles, different proportions of jobs at different levels in the hierarchy, etc.) The experiment we shall describe here relates organizational development and performance in laboratory groups to variations in the historical sequence (not the universe) of the tasks they have worked upon.

Concern about the organizational effects of task factors has only recently received much attention. Lanzetta and Roby (1957) have been working with the effects of such task variables as amount and distribution of information on group performance. And very recently, Newell, Shaw, and Simon (1958) have shown a close relationship between the *order* of presentation of problems and, in effect, the rate of development of generalized problem-solving ability by a computer program.

The earlier research on communication nets (Christie, Luce, & Macy, 1952) is also relevant here, for those experiments demonstrated a relationship between task and structure by showing the relative inflexibility of some structures when faced with changes in certain aspects of their tasks. In general, however, the networks experiments held task constant and varied communication structure, observing the effects of such variation first on performance and later on organizational development. In the present experiment, the constancy is shifted to communication structure, which is kept not only constant but nearly minimal. The task is varied. Observations are then taken on the relationship between task variations and processes of task-solving and organizing for task-solving. By "organizing" we mean defining and utilizing rules for making appropriate task-solving decisions when a task is at hand.

Effects of target order: Some hypotheses

In the absence of direct intercommunication, the *order* in which particular targets were presented in the Common Target Game would, we hypothesized, influence (1) the group's performance (the average number of efforts required to hit test targets), and (2) their program (the nature and degree of categorization of targets; the nature and degree of differentiation of roles; and the nature and degree of role allocation.)

More specifically we predicted the following:

1. 4-man groups put through an "ascending" (as viewed by E) series of targets (targets of the category $4x$; then targets of type $4x + 1$; then $4x + 2$ targets; then $4x + 3$ targets) would perform better than groups put through a "descending" series (the same targets in reverse order, starting with $4x + 3$ and "descending" to $4x$).

2. Groups working through an ascending series would more frequently achieve a more nearly perfectly functioning program than groups first put through a descending series.

3. The variety of program alternatives introduced (alternative target categories and alternative ways of distributing roles) would be smaller for ascending than for descending groups.

4. The ascending groups would more frequently organize around the class $4x \pm r$, and within all possible variations of such categories, around the $4x + r$ subset.

5. The ascending groups would more frequently utilize "concentrated" than "deconcentrated" role systems.

EXPERIMENTAL PROCEDURE

The following target series were set up for 4-person groups: For "A" (ascending order) groups, 16 pretest targets in the order: 24, 16, 12, 8; 9, 13, 17, 25; 26, 18, 14, 10; 11, 15, 19, 27. For "D" (descending order) groups, the same 16 pretest targets, but in the reverse order: 27, 19, 15, 11; 10, 14, 18, 26; 25, 17, 13, 9; 8, 12, 16, 24. For *all* groups, after the pretest, we appended the

following 18 test targets in this order: 30, 21, 7, 1, 39, 22, 29, 31, 2, 38, 6, 33, 3, 5, 35, 34, 37, 23. Note that the pretest targets are arranged into four subsets of four targets each. Subset 1 is made up of $4x$ targets; subset 2 of $4x + 1$ types, etc. *A* groups first received the $4x$ targets then "ascended" to $4x + 1$, etc. The *D* groups started with the $4x + 3$ targets, and "descended" to the 4's with residual 2, etc. The 18 test targets that followed this initial series were not random either. They were all new targets, designed to give equal representation to the classes $4x + 1$, $4x + 2$, $4x + 3$; and selected to include extreme targets close to the top (40) and bottom (0) ends of the range. *S*s were run through all 34 targets consecutively, without a break and without discussion. 27 groups were run, 14 *A*'s and 13 *D*'s.

The 14 *A* groups consisted of eight groups of graduate students from the University of Chicago, predominately from the School of Business; four groups of Air Force officers (majors and lieutenant colonels); and two groups of undergraduate girls, all members of the same social club. The 13 descending groups were drawn from the same populations, with seven groups of students, four more groups of Air Force officers, and two more groups of undergraduate girls from the same club. Non-military *S*s were paid $1 each in advance of their participation. There was a good deal of shifting around and last-minute adjusting of groups, so the probability is high that members of some student groups knew one another quite well, while other groups were made up of relative strangers. Members of four of the Air Force groups, two *A*'s and two *D*'s, had been working together for a year; the other four groups had been working together for about three months.

*S*s were seated around a square table, one on each side, backs to the table. Each was supplied with eleven 8½″ x 11″ cards held together by loose leaf rings, with the digits 0 through 10 appearing sequentially, large and black, one on each card. Instructions were uniform, requesting *S*s to work toward a system that would allow them to hit any target in one effort. Once under way, *E* called out a target number, waited a few seconds and said, "Up cards." *S*s then raised their cards with the integers they had selected so that all cards faced *E*. *S*s were unable to see one anothers' cards. *E* then repeated the target, announced the sum actually achieved by the group, and then announced the distribution of individual contributions (without identifying individual contributors). If the group had hit the target correctly, *E* then said, "Your next target is. . ." and the experiment continued. If the group had missed the target, *E* then said, "Will you try again for target *X*?"

For purposes of analysis, we call the first 16 targets "phase 1"; the next 14, "phase 2"; and the last 4 targets, "phase 3" of the experiment.

After the 8th, 16th (the last of the pretest targets), 21st, and 34th targets, *S*s were asked to stop and write a few sentences describing the procedures they thought other people were following and the procedures they were following. The instructions here were kept loose. *S*s were encouraged to write whatever they had been thinking about. *S*s were interviewed, informally, after the experiment.

RESULTS

Degree and kind of organization

We predicted differences in the degree to which systematically operating programs would evolve.

One gross measure of systematic operation could be gotten from runs of targets hit in a given number of efforts. Using the last four targets as our criterion, we find that four groups of the total of 27 hit each of the last four targets in one effort. These were all *A* groups. One *D* group hit each of the last four targets in two efforts.

But our most useful data on degree *and* kind of organization come from analysis of patterns of contributions by group members, and from *S*s' protocols describing their own behavior.

Consider the following data (Table 1) from one of our *A* groups over the last *six* test targets. (Note that these data are from a group that hit each of these targets in just one effort.)

TABLE 1
MEMBER CONTRIBUTIONS FOR ONE GROUP OVER LAST SIX TARGETS

	Target number					
	3	5	35	34	37	23
Member A contributes	1	2	9	9	10	6
Member B contributes	1	1	9	9	9	6
Member C contributes	0	1	8	8	9	5
Member D contributes	1	1	9	8	9	6

We sought the best fit between these data and our models of possible combinations of category and role-systems. It seemed clear that member *C* regularly contributed ¼ of the next number below target evenly divisible by 4. Member *D* did the same, except that when $T = 2x + 3$ he added one to his contribution. *B* picks up a piece of residuals of two or three, and *A* adds one if there is any residual at all. (But note that *A* may, in fact, have simply been following the rule of taking ¼ of the next higher whole dividend.)

We, therefore, regarded the performance of this group as perfectly describable by our model of a $4x + r$, deconcentrated program, even though member *A*, for example, may not have perceived his own performance as we did. The protocols from this group were searched for substantiation or contradiction of our fit between data and model. They supported the analysis. Here, for example, is a portion of one protocol, taken after the 21st target. *S*: "The group continues to solve the problem by contributing only two kinds of components at any one time, i.e., (x) and $(x + 1)$, differing no more than one integer. If three like components are required, I contribute one of these. If two sets of two equal components are required, I join the set contributing the larger component."

Extending the same kind of analysis to the other three perfectly performing (all *A*) groups we find the following. The behavior of three groups (including the one just described) could be described by the $4x + r$ categories, deconcentrated model. The fourth was best fitted by a $3x + r$ categorization with a set of subrules for targets over 30; three men taking whole dividends, and the fourth taking only residuals.

Using the same process on all other groups, and labelling as "unclear" all groups in which the behavior of fewer than three members fitted one of our models, we arrive at the frequencies of category-sets and role-distributions shown in Table 2. It can be seen that *A* groups use divide-by-four categories somewhat more frequently than *D* groups (9 *A*'s against 4 *D*'s), in accord with prediction.

The behavior of a large number of groups was not observably clear enough to be pigeon-holed. Among the clear groups, *A*'s appear to be using a greater variety of organizations than *D*'s, contrary to prediction. But a survey of the protocols reveals far greater variety of *individual* approaches among members of the *D* groups.

The single more or less effective use of a 10's program occurred in one of the *D* girls' groups. It is clear from the protocols that some participants in that group did not know that they were doing what we think they were doing and some ambiguity remained in the organization even at the end of the run.

Adjustment

We counted, in our analysis of protocols, mentions of *adjustment*. A mention of adjustment was any reference in the protocol to *adjusting* or *modifying one's contribution as a consequence of the behavior of other subjects*. (No subject was scored with more than one reference to adjustment.) Here is a portion of a protocol scored as "adjust-

TABLE 2
NUMBER OF *A* AND *D* GROUPS WHOSE BEHAVIOR FITS PARTICULAR COMBINATIONS OF CATEGORY AND ROLE SYSTEMS

Categories	Roles	A's	D's
4's	Deconcentrated	4	4
	Semi-Concentrated	2	0
	Concentrated	1	0
	Unclear	2	0
3's	Concentrated	1	1
	Unclear	1	0
10's	Deconcentrated	0	1
Unclear	Unclear	3	7

TABLE 3
EFFORTS/TARGET ANALYSIS OF VARIANCE

	Sum of Squares	DF	Mean Square	F	P
Among Phases (1st 16 targets, 2nd 14, last 4)	.86	2	.43	—	—
Between Target Orders	4.82	1	4.82	6.86	>.05
Interaction	1.38	2	.69	—	—
Residual	55.18	72	.77	—	—

ment": "Hold number for x tries until I determine how many are varying theirs. They try to determine how they are varying theirs; then compensate."

This tendency to perceive the problem at least partially in "local" feedback terms occurred considerably more frequently in D groups than A groups. Some mention of adjustment was made by 20 S's who were members of 10 of the 13 D groups; and by 11 members of seven of the 14 A groups. Moreover, within A groups, there is a relationship between tendency to mention adjustment and poor performance. Thus adjustment is mentioned by just one member among the seven top performing A groups (performance = mean efforts/target over all 18 test targets), and by 10 members of the seven low-half A groups.

Performance

Efforts/target (number of trials to hit target exactly) constitutes a ready measure of group performance. Our prediction of superior performance by A groups was tested first by two-way analysis of variance, grouping the scores according to "phases" (mean efforts/target for the first 16 targets, the next 14 test targets, and the final four test targets), and orders A and D (Table 3). Data from one A group drawn at random from the eight student groups were discarded to permit matching of X's.

Only differences in target order showed significance in this analysis. The differences favor the A groups but not unequivocally, as is shown by the means and ranges (of group means) of efforts/target (Table 4).

TABLE 4
MEANS AND RANGES OF EFFORTS/TARGET

	First 16 targets	First 14 test targets	Last 4 test targets
A.			
Mean	2.1	1.7	2.0
Range	1.3–3.4	1.1–2.9	1.0–4.5
D.			
Mean	2.8	2.7	2.2
Range	1.4–4.4	1.3–4.9	1.3–4.5

Three other points are worth noting here. First, a median test on all 18 test targets picked up a significant difference between A's and D's only on the *first* target in the series. But second, for 14 of the 18 test targets the mean number of efforts for A's is lower than for D's. Third, during the pretest period the first four A targets (all were divisible by 4) were respectively the 16th, 15th, 14th, and 13th D targets. The A mean was 1.6 efforts/target; the D mean (after 12 experiences), 1.8. Similarly, the first four D targets (all with residuals of three) were the 16th, 15th, 14th, and 13th A targets. The D mean for these four was 3.6 efforts/target, the A mean only 1.9 efforts/target.

DISCUSSION

We account for the initial differences in performance and degree of organization as follows: The use of initial targets evenly divisible by n increases *prominence*, in Schelling's words (1957), of the idea of division by n. Prominence here means two things: First it means that the idea of division by n will occur frequently to individuals to whom the problem of n players and targets divisible by n is described (even if the individuals are not really playing the game with other players). Prominence also means that the idea of division by n will occur frequently and additionally to individuals who *are* players because they will assume that other players will also perceive division by n as "reasonable"—probably because it is symmetrical, i.e., it provides approximately equal "shares" to all members. Hence initial organization around categories. By this view an initial series of targets evenly divisible by 10 should *not*,

for a 4-man group, be as effective in speeding up organizational processes as a divide-by-4 set, because although 10 fingers match nicely with targets divisible by 10, the 10's system sets asymmetrical, non-egalitarian roles for the members. We suggest that deconcentration, which occurred frequently in both experimental orders, probably occurred mostly for the same reason; and not because *Ss* foresaw the task limitations of concentrated programs.

Turning now to some other findings, it seems impressive that so few groups got themselves fully programmed by experiment's end. In our preliminary runs, we had to search again and again for ways of making the task easier so that we could get at least some of our groups operating at high efficiency. We can suggest two reasons for the generally low degree of organization. One of them clearly is the restriction of direct intercommunication. But another was a tendency in many groups to reach an early impasse in selecting one set of categories from among several offered by different members. It was as though reasonably intelligent individuals each saw a reasonable solution to the task, but having found it, could not be induced to perceive that alternatives offered by others were almost as reasonable. If members had been permitted to intercommunicate, these unstable stalemates would have been easily removed.

Finally, we think that one of the most curious and enlightening occurrences in the experiment was the discovery that a small number of *Ss* defined the task differently from the rest. While most *Ss*, given the task, proceeded to categorize targets and then to go on to other parts of the program, some *Ss* did not. They treated the task first from a *social* perspective, writing in their protocols of "adjustment," "leadership," et al., and never mentioning division of the targets by an integer. Most of the groups with such members performed badly, but some showed improvement and some ended up performing moderately well.

It is possible that number-fear had something to do with this behavior—that these *Ss* (mostly women) blocked at the idea of doing arithmetic, retreating instead into a social treatment of the problem. But this explanation, whether true or not, implies a hierarchy of appropriate ways of attacking this task—a hierarchy in which this social attack would rank lower than an arithmetic one.

Such a hierarchy is not easy to defend, either from our data, or from the logic of the problem. For although no such group appeared in our sample, we might theoretically have gotten a "local" working program without *any* target categories. One example could be a local feedback program, that could solve any target in *two* efforts by calling for random behavior on effort #1 and corrective behavior (rules like "High man will reduce if we are over target; low man will come up if we are under; plus appropriate subrules) on effort #2.

Such an inelegant solution based on previous behavior of members ought not be judged too quickly as less rational or less good than *one* effort programs; for such a local program might prove to be more adaptable to, say, changed tasks than one effort programs. The divide-by-*X* programs, after all, are highly task-specific; their accompanying role systems and assignments are utterly dependent on the categories selected; and the categories in turn are utterly dependent on the specific task. But the local program is *relatively* task-free, and is bound instead more closely to the people who make up the group.

REFERENCES

Christie, L. S., Luce, R. D., & Macy, J., Jr. *Communication and learning in task oriented groups.* Res. Lab. of Electronics, MIT Tech. Report (No. 231), 1952.

Lanzetta, J. T. & Roby, T. B., Group learning and communication as a function of task and structure "demands." *J. Abnorm. Soc. Psychol.*, 1957, 55, 1.

Newell, A., Shaw, J. C., & Simon, H. A. Elements of a theory of human problem-solving. *Psychol. Rev.*, 1958, 65, 3.

Schelling, T. C. Bargaining, communication, and limited war. *J. Conflict Resolution*, 1957, 1, 19–36.

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The structure of a "balance of power" system can be explored with the aid of models, but for practical and theoretical reasons, the theorist often has difficulty in displaying all the implications of his model. In an effort to bring out and examine the stabilizing qualities and other potentialities attributed to the "balance of power" system, the authors of this article have invented a competitive game of international politics—not as itself a model, but as a device for "playing out" models and theories.

THEORETICAL ANALYSIS OF THE "BALANCE OF POWER"

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If one thinks of a "balance of power" system of independent nation states as being somewhat like the system that actually existed in the eighteenth and nineteenth centuries, some fascinating theoretical problems present themselves. Is the system stable—that is, does it have the property that it tends to be maintained as a system of independent nation states, characterized by groupings into rival transient alliances; or does it tend to be transformed into a different system, either by a reduction in the number of states or by aggregation into a system of relatively permanent alliances? Or will the system be unstable or stable depending upon the conditions or assumptions built into the system? Is there some lower bound upon the number of nation states necessary for stability? An upper bound? Since stability obviously depends in part upon the behavior of the members of the system, what motivations or mechanisms trigger the behavior that makes for stability?

Obviously these questions cannot be answered in terms of a model that attempts to account for *all* the factors and processes operating in the real world. The science of man, society, and the environment as interacting systems is too complex for theoretical treatment. If, however, one examines the "balance of power" international system in terms of a restricted set of variables, it may be possible to build a theory of the system and to examine its internal mechanics.

Several such theories and models derived from different sets of variables have been developed—by this article's authors, amongst others. For illustrative purposes, one particular model is sketched below. If we find that, given appropriate inputs, its workings reproduce *some* features of the historical balances of power, we shall try to make it work also under varying conditions, so as to bring out and examine the stabilizing and other potentialities attributed to the "balance of power" system.

To do this, however, we need a method for deriving the implications of any model—a way of 'setting it to work'—less time-consuming than that of formal inference. We have therefore invented a *competitive game* of international politics.[1]

THE GAME

The game is not a *simulation* of any actual political process, nor does it directly provide a theory or model of world politics. It is rather a means of displaying the formal properties of any one of a family of models—viz., of those which assume that the "balance of power" and other international systems are (i) essentially competitive and cooperative at once, (ii) essentially responsive to military force or the threat of it, and (iii) sensitive in the long run to changes in their constituents' economic potentials, from

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[1] For rules of the game, see the Appendix.

which alone their military forces are produced.

The object of our game, therefore, must include both competitive and cooperative ends, though the proportion or mixture of these should be variable. The rules must allow for the possibility of making war, though the rate at which the embattled forces exchange should again vary with the requirements of the model to be displayed by means of the game. (The stability of any balance of power, and the motivation it affords for alliances and other such balancing alternatives to war, are acutely sensitive to the form and intensity of the military exchange rate, and to the power of weapons to destroy economic resources.) And last, the equivalents for 'military force' to be displayed in the game must flow in the last resort from something equivalent to the instruments of production—the rate of that flow, again, may be varied.

The rules concerned with those three elements in the game, and any other rules introduced, must cohere so that a conflict of rules *never* arises in any play of the game. We cannot of course be sure that the particular set of rules in the Appendix is thus completely self-consistent, but we believe that a tolerable number of further plays can help us make it so.[2]

Since, as we have suggested, various sets of rules are possible within the limits of self-consistency, our device might more aptly be called a family or genus of game. Its various possible forms are related to each other somewhat as are auction bridge, contract bridge, and solo—a family of card-games evidently distinct, for instance, from the various forms of poker.

The game (or games) is adaptable in another way. Linked to some types of model, it will best be played as an ordinary table game, and perhaps only so. With models involving contrary assumptions, e.g., those which require that 'players' follow specific strategic maxims or adopt definitely prescribable styles of play, mechanical techniques for running the play are probably more desirable. Pencil and paper studies of the game are also possible, as with problems in chess and bridge.

If chess or bridge is to be thought of as having, in principle, game-theory solutions, it seems intuitively obvious that any self-consistent species of our game will have a solution in principle. We understand, however, that the theory of games for any number of players greater than four has yet to be developed. It is also evident that the permutations of strategy in all but the simplest and least interesting two-person variety of the game must put it beyond the reach of any calculator. Though it should be possible to teach the "balance-of-power" game to machines capable of learning chess, we do not at present see much point in doing so.

"BALANCE OF POWER" MODEL

Having described our game as a device to display the implications of models and theories about international systems, we shall now refer to one such supposed system—the "balance of power"—and to a model intended to reveal and reproduce the mechanism which "balances of power" are supposed to have embodied.

Both Kaplan and Burns have made attempts of this type previously; it is now desired to construct a somewhat more sophisticated model of the "balance of power" and to examine the conditions under which it may be possible to test some of the consequences of the model by use of the machinery of our game. One of the important variables of the model will be the nation state which, in this model, will not be differentiated internally. We will also assume that there are no political impediments to the decision-making process and no constraints on the utilization of the nation's resources except economic constraints. Moreover, the information of the decision-makers will be complete. That is, they will know how much in the way of economic and military resources the other nations have, at least at the present time. They will not

[2] We are already much indebted to the members of the Princeton University Center of International Studies in particular and of Woodrow Wilson School in general, who spent a number of evenings playing the game so that contradictions in the rules might be exposed. The Center also provided the equipment required. It cannot have been easy to show that the purchase of a hundred and fifty dice would contribute even indirectly to the study of international politics!

know, of course, what decisions other nations are likely to make except insofar as information concerning this is volunteered during a bargaining process. The nations will each have one and only one frontier with each other nation. For more than four nations this is not physically possible geographically, unless one includes sea or air frontiers; yet one could construct a space in which this would be possible. In addition, each nation can move any portion of its military resources to any frontier with equal ease or difficulty. Military resources will not in this case be nuclear. The military exchange rate will be specified in the game to be introduced later, as will the absolute and relative amounts of military resources. They will be assumed equal, however, at the beginning of the game. After each round of the game, there will be increases in national productivity. These increases will be unequal and the ratio of increase per productive unit will be determined by chance. Productivity increases can be transformed into military resources in a way to be specified later. We will further assume, at least as a preliminary assumption to be varied later, that each nation dislikes uncertainty, that is, that it prefers its present situation to a risky gamble that would improve its position, but would also prefer a still less risky situation to its present one. It is also assumed that each nation desires to maintain its position as an independent nation state, that is, that no economies of scale or values dictate either absorption into a larger organization or sacrifice for a different nation. Yet no nation can be sure that other nations lack a positive incentive for war or will lack such an incentive in the future. Such incentives might include a desire to rule other nations, a desire for supranational organization, a desire to overcome internal political stresses, or other reasons. Almost all the previous assumptions are contrary to fact; yet, as we shall see, they permit us to investigate economically the properties of the "balance of power" international system.

THEORETICAL ASSUMPTIONS

Before investigating alternative explicit models of the various systems consistent with the assumptions, discursive analysis of the theoretical problems arising from the assumptions will provide insight into the problems the explicit models are designed to illuminate further. Each of the nations, as it observes the world in which it operates, observes certain fundamental uncertainties which make that world somewhat hazardous for it. Other nations may grow stronger than it because of internal economic reasons, that is, the incidence of stochastic increases in national productivity may operate against it, or other nations may combine against it to increase their security. Since, in this discursive model, the internal economy cannot be manipulated to guard against declines in security occurring for other reasons, it is important either to enter an alliance or combination strong enough to guard against unfavorable stochastic increases in productivity and against the pressure of military build-up on one's frontiers or to go to war in order to force a favorable change.

Formation of alliances

In a world of five equal nations, for instance, an alliance of three would enjoy a reasonable degree of security, the counter-alliance of two being unlikely to win a war. One might assume that there would be sufficient security in the alliance of three not to necessitate a war. However, there is no reason to assume that the alliance will endure indefinitely. If the assumptions of the model are even a little extended, some contingency arising from an insult, from political events that have external consequences, and so forth, is sure to arise and to upset the alliance. There is a strong temptation to go to war in order to increase one's national strength in relation to at least some other nations. There are additional reasons for this. A favorable combination of gain from war and gain from stochastic increase in productivity might make it possible for a particular nation to gain equality or better in an alliance of two rather than three. In this case, one would need to find only one rather than two alliance partners to protect one's security. In addition, if the stochastic gains in productivity turned out to be unfavorable, a gain in war would decrease the possibility

that two other nations could form an alliance that was superior to the alliance the first nation was in, even though it has two partners. Moreover, in one sense, a gain in war might make it easier to gain alliance partners in the future, since, being more than equal in capability, one might be able to offer potential partners slightly superior benefits for joining one's alliance. For these reasons we may state as a first hypothesis about the model's working that there is some tendency in the system for alliances to form and for alliances to culminate in at least limited wars.

Counter-alliances

Let us examine the process of alliance formation further, however, to see whether the tendency to form alliances might not be counter-balanced by even stronger considerations. Suppose, for example in a three nation system, that nature establishes conditions that permit one nation to make the first offer of alliance, that it seizes this opportunity, that the offer is a "fair" offer in the sense that any gains from the alliance are to be allocated equally and any costs shared equally, and that the imputation established by the offer is dominant in the game theoretic sense. We shall consider the following hypothesis about responses to that offer. The nation to whom the alliance is offered knows that it can get a better deal from the nation that is left out. Since that nation does not want to be on the losing end of a war, it will accept less than a "fair" offer or enter an imputation that is not dominant. If, however, this new alliance is formed, it can be replaced by an alliance of the nation left out in the first imputation and that left out in the second, for the nation left out in the second imputation can offer a better deal to the nation left out of the first imputation than it gets in the second. That the offer will be accepted is likely, not only because it is better, but because the other nation in the imputation has already established a reputation as a "s[illegible]per." Since that nation can be bought off it is important to anticipate him. Thus the [illegible]n offered the alliance originally mus[illegible]k being left out in an unstable bargaining process if he refuses the offer. And, as the offer is "fair," he has no reason to do this if he is reasonably conservative. Similar reasoning indicates that the nation making the offer will not listen to a better offer from the nation left out.

Suppose, therefore, that an alliance is formed and suppose further that it goes to war against the third nation. Now suppose the third nation threatens to commit all its forces against one of the alliance partners and to surrender to the other. If this threat were credible—and it might be made credible by a proper distribution of the forces—the nation against whom the threat to commit the forces is made would offer an alliance to the threatening nation. Since the same thing would happen whenever an alliance was formed and prepared to engage in war, war never would occur, unless, at the end of a series of stochastic changes in national productivity, two weak nations went to war against one strong nation.

That such a "standoff" would occur is unlikely, however. One reason is obvious, but our model excludes employment of that reason, namely that the public would not stand for the threat of suicide, no matter what its practical advantages. However, there are other reasons why the threat is unlikely. One cannot assume perfectly rational behavior. If the threat were made, the proper dispositions assumed, and the threat turned out to be ineffective, the consequence would be disaster. The victorious nation would reap the "spoils" and immediately turn on its weakened erstwhile partner. On the other hand, if the lone nation fought and were defeated, the defeat might be only a limited defeat (this consequence is much more likely when there are more than three nations in the system for reasons that will be clarified soon). It is less likely that two nations would expect to impose a harsh peace than that a single nation would impose such a harsh peace. A two nation system seems inherently unstable because the stochastic increases in productivity would soon give one nation an edge on the other. Therefore both victorious nations have an interest in limiting the damage inflicted on the defeated nation and maintaining it as a

potential "balancer" for the system. Moreover, both victorious nations would have to agree to a harsh peace and the defeated nation has some bargaining power if it does not surrender.

It might seem (contrary to the previous assumption by the authors) that a three nation "balance of power" system would be stable, that some nations would not tend to be eliminated or reduced to insignificant strength. However, this is unlikely. Although the two victorious nations have an interest in limiting the defeat of the third and in restoring it to the system as an acceptable role partner, they might not do so. Since this might not happen, the penalty for being left out of an alliance would be high and even the hazards of being in an alliance relatively great. Even if a nation were in one alliance, it might be left out of the next. Therefore this would be a system in which each victorious nation might attempt to gain as much as it could from the war as a protection against what might happen in the next round. Moreover, each victorious nation would attempt to double-cross the other in order to obtain a differential advantage. There would be a premium upon deceit and dishonesty. On the other hand, the addition of some other nations to the system would remove many of the pressures and thus add to the stability of the system.

The discursive analysis would seem to support five of the essential rules of Kaplan's "balance of power" system (1957, pp. 23–24). It suggests that the nations rationally would attempt to increase their capabilities; would go to war, if necessary, for that purpose; would form alliances and counter-alliances; would limit the spoils of war (given a sufficient number of nations in the system); and would treat former enemies as acceptable role partners in future coalitions. A sixth rule, specifying that nations combine against a nation that has supranational ambitions, does not seem to be supported within the framework of the specified assumptions.

Problems of supranational nations

The question is: Why should a nation be singled out for opposition just because it has supranational ambitions? Should not the normal operations of the system, that is, the process of alliance and counter-alliance, serve to control it? If we were to assume that a nation with such ambitions had a special facility for making foreign policy decisions quickly and efficiently, then it might be that more slow-moving nations could counter its activity only by entering a sort of protective league. Or it might be that the nation with supranational ambitions has a clearer knowledge of its goals, in which case other nations learn how to oppose it only when it requires their combined strength to make opposition effective. Or it might be that nations with supranational ambitions also are able to make use of organization, for instance, a supranational party, or ideology, to weaken their opponents internally, in which case the normal essential rules of the "balance of power" system are difficult to effectuate; in this case, combination is essential. Finally, the problem may involve any harsh peace and not that alone imposed by a nation with supranational objectives. It might be that a harsh peace has such unstabilizing effects on the "balance of power" system that all other nations have a joint interest in opposing this, and may also have a joint interest in punishing the nation which attempts it in order to reinforce the rule dealing with the limitation of objectives in war.

Thus, although we assume that alliances are fragile and that nations will tend to drop out of victorious alliances because, if overwhelmingly victorious, they will become threats to at least some of their own members, it may be that a general coalition is required against a nation which can manipulate organizational or ideological advantages, which has made extensive gains because other nations have not behaved according to the rules, or which attempts actions that have an unstabilizing influence upon the international system.

STABILITY OF A "BALANCE OF POWER" SYSTEM

We do not yet know the number of nations that is optimal for stability. Three clearly would appear too low. Just as clearly some upper number would be too large. For in-

stance, if there were 2,000 nations within the same relative capability range, the elimination of several would not necessarily be a danger to the others unless they were eliminated by a single nation which began to gain certain economies of scale that did pose a threat. Kaplan and Burns agree that five is a minimal number of nations for stability. Kaplan, however, thinks that the gain from having potential coalition partners is still great enough above the number five to justify some additional number of nations. Burns, on the other hand, believes that additional numbers generate problems of complexity in decision-making that outweigh the gains from potentiality of alignment. Quandt believes that five is not necessarily minimal for stability.

This question about the numbers of nations most apt to provide a stable "balance of power" is a specification of the following more general questions: is any "balance of power" finally stable; and if it is, is it stable for purely structural and mathematical reasons? The authors' answers to these latter questions are partly divergent, and thus recapitulate, to some extent, historical controversies concerning the "balance of power."

Upon its behalf, its early supporters claimed that the selfish ambitions and desires of rulers for power and security served, despite them, to conjure up an international order which tended (i) to provide an alternative to war and (ii) to prevent any one nation's achieving hegemony. Sometimes they also alleged (iii) that it preserved weak and small nations from conquest. In the later stages of its history, "balance of power" became a normative concept—maxims of political wisdom included adjurations to "strive to maintain the 'balance of power.'" But originally a 'hidden hand' (i.e., a structure finally analyzable in quantitative terms, not the creation of any human being, and functioning quasi-providentially) was thought to maintain it—strive as he might for his own power and security, the ambitious ruler could achieve nothing but a "balance" with others that at once preserved and restrained him.

The 'hidden hand' version makes a bold, striking, and powerful hypothesis, especially when it involves the claim that no nations, however weak, will be eliminated—a claim that is, to complete stability. It is therefore worth testing. Even in this weaker form—that given an optimum number of member-nations and a favorable state of military art, the "balance of power" will prevent the elimination of members—the equilibrium hypothesized would call for analysis which the 'playing out' of a model might provide.

Less theoretically surprising but closer to much historical reality is the view that a "balance of power" tends to be stable (by which we shall now mean that even weak or dependent nations tend not to be absorbed by others, or physically destroyed or partitioned in wars) when all the member-states comply with a conserving or legitimist code. That is, while each seeks to improve its own position in the system, it still observes certain limitations and duties which serve to maintain the system and to keep up the number of its members. If such a dual policy could be worked out so as not to produce conflicts of strategic maxims, the system in which all followed that policy would seem a priori more apt to be stable than would the Hobbesian system of the last paragraph.

The essential rules of Kaplan's "balance of power" (see above) formulate the policy which, if generally adopted, would promote such a system. A model of it might be 'played through,' either by instructing human players to adopt a broadly conserving or legitimist style of play, or by programming a machine with strictly quantitative rules (e.g., never to allow a defeated nation to be reduced in potential to a level at which any other unaided nation alone would have a high probability of defeating it again). If the latter method were to be adopted, the system's expected stability could be subjected as a test to severe strain from other sources—great advantages for surprise attack might be established in the military exchange rate, and rate of production might be allowed to vary more widely. It would not be easy to ensure that human players consistently balanced their self-preserving and system-preserving objectives, and therefore machine-play would better

help explore such a system's stabilizing properties. Once these were determined, however, it might be interesting to see whether people trying to adopt the legitimist style of play might not turn out to behave paradoxically, so that either the self-preserving or system-preserving motivations proved self-defeating. Or it might turn out that system-preserving activity was also self-preserving in which event the legitimate norms of the system might only represent the learning behavior of the actors.

ADVANTAGES AND LIMITATIONS OF THE GAME

Since we are supposing that rigorous game-theoretical solutions are practically out of the question, it is clear that any experimental evidence related to a given model's propensities to stability, alliance formation, or incidences of war would have to be statistical. This would require careful experimental design, e.g., in determining the number of plays required to test an hypothesis. Information would be most economized if the playing-out of the several models under study were designed together so as to remove accidental variation in the assumptions not under test. This seems to entail a large enterprise, and so the authors look for discursive analysis and criticism of their scheme and methodology, before embarking on model-testing plays of the game.

They also recognize how limited in scope, nature, and bearing on the real world must be any information derived from competitive gaming. If, for example, games in which no style of play was required, and in which the players early divided into two equal sides, turned out to be less stable than those in which there had been three or more coalitions of comparable size, this would not show that in the real world bi-polar systems are necessarily the less stable—it would merely leave the old 'mathematical' claims on behalf of the "balance of power" still unrefuted. Similarly, a successful stabilization by competitors instructed to adopt a legitimist style of play would be at best suggestive about legitimism, i.e., that it was an almost Darwinian adaptation in eighteenth and nineteenth century Europe. As is so often the case, refutations of hypotheses about the model (results, e.g., that showed no correlation with numbers of nations in a system, or with styles of play) might be more informative, since they would at least rule out old beliefs about the "balance of power." We wish to make it very clear that our gaming devices are meant to aid the theorist in the understanding of his own models, and not to serve as interpretations of the real world.

Our competitive games are not aids for the *teaching* of international politics. Such a claim can be and is made on behalf of simulation games, especially those which involve a large number of qualitative disparate elements which the role-player must bring into play. The rules of our game, by contrast, are designed to reduce qualitative variety. Further, the experimental playing of it is better conducted by persons who do *not* think of it as in any way analogous to international politics. It may well be desirable to recast the terminology of the rules so as to eliminate misleading comparisons.

On the other hand, there are certain small-group situations, not necessarily connected with international affairs, which our game and several other competitive games may help to illuminate. The formation of 'alliances' in our game is an instance of the making of a compact or bargain strengthened by reciprocal guarantees, while the allocation of forces against another player represents a threat, with supporting commitment. It may be a matter of psychological interest, and suggestive for bargaining theory, to see whether offers of alliances, for example, are made by the weaker or the stronger parties, and what rationale is most often provided to account for the choice. Well-kept records of experimental games may be informative along those lines.

POSSIBLE EXTENSIONS OF THE GAME

Many additional features can supplement our game so as to extend its usefulness to the explicating of theories about international systems more highly developed than those so far discussed. Each player could

use in fact a group of persons related to each other according to some model of an internal political system, democratic or not. A competitive business game could replace stochastic determination of the rate of national income. Geographic constraints could be introduced instead of each player having a frontier with every other player; the game could involve a map with arbitrary or geographically appropriate frontiers. Movements of forces and resources could be costly according to distance, etc. The interrelation of weapon-systems could be much more complicated. Complete or perfect information is not an unavoidable assumption—reserved and deployed forces, and also resources, could be concealed more or less, and communication with other players could be kept secret. An international banking system might be set up. An effect like that of the qualitative arms race could be produced by alterations in military capability, such as those suggested in rules 5 through 10 of the game outlined in the Appendix. Eventually, all processes could be automated so that the constriction of discrete 'moves' and 'rounds' would give way to continuous playing, which would introduce something like the urgency and strain of political affairs.

We may now append a specimen set of rules, the first four of which could appropriately link with our discursive model of the "balance of power" described above. It must again be insisted that terms like "balance of power," 'forces,' 'resources,' 'war,' and 'spoils of victory' are not meant seriously to refer to the real world, but to suggest to the reader possible theoretical uses, in conjunction with other models, for games such as this one.

APPENDIX

by Arthur Lee Burns, with the collaboration of Richard E. Quandt and Morton A. Kaplan

Brief description of the game

The players are Nations competing for pieces (chips or dice). Pieces represent units of resources; or they can be set aside as reserved forces which, when 'deployed' on 'frontiers' against other Nations, become 'sources of fire-power.' Undeployed resources, i.e., those neither deployed on frontiers nor in the reserve of forces, earn proportionate income, but in proportions that may vary by chance from round to round.

Players take turns to move. A move can be passed; or it can be used to deploy, reserve, or withdraw forces, or to 'make war' with forces already deployed. If at war, opposed pieces on a given frontier exchange at a rate determined by throwing the deployed dice and do so until one side's pieces deployed on that frontier are eliminated. The regular sequence of moves is suspended by warfare, until the initial conflict or battle is finished, the victor in that battle has taken some pieces from the defeated, and each contestant has had the opportunity to re-deploy and to make war. Players know either that the game will be played to a finish or after a specific number of rounds it will conclude with a payoff which (in either case) will depend on the number of pieces then possessed.

'Alliance' is supposed to consist in two or more Nations' reciprocal withdrawing of forces from a common frontier(s). 'Pressure' consists in a Nation's increasing the forces it deploys against another. Degree of pressure is increasingly a matter of predominance at specific frontiers because of the Lanchester-type exchange-rate in war.

Object of the game

The object of the game is to have as many pieces as possible at the end of the game. If a player finishes with a total of pieces, deployed and not deployed, fewer than some number decided beforehand by the players[3], he shall be deemed to have

[3] A good and theoretically significant game may be obtained by relating the minimum payoff to the number of moves played, e.g., a player gets nothing if he finishes with fewer than the number he started with plus one for every two rounds played.

scored zero, and such pieces as he has shall be put into kitty, which shall then be distributed amongst those who have at least as many as they started with, in proportion (to the nearest whole number) to the totals they finish with. The players shall jointly determine beforehand the time, or the number of rounds by which the game is to finish. When more than two are playing, the game will be more exciting if it continues for a number of rounds greater than the number of pieces initially in kitty divided by the number of players. The fewer the players, and the earlier in each game, the shorter the time taken by a round.

A more 'realistic' variant could be obtained by agreeing to play until *either* all but one are eliminated, *or* a unanimous agreement to stop is reached—bribery being permitted if it brings about agreement. Players could severally pay for their initial pieces, and jointly for the initial kitty.

Rules of the game

(1) Each player begins with 8 pieces plus a number of pieces equal *either* to 7 *or* to twice (the number of players minus 1), whichever is the less; and he drops out of the game if and when he has no pieces left. Kitty may contain any number of pieces; but a desirable number would equal the total of all the players' initial pieces, or about one and a half times that number if there are only two or three players.

(2) At the conclusion of every move, each player receives an income calculated on the following basis. For every 8 pieces which, throughout the previous round, he has *not* deployed on frontiers, he receives either 0 or 1 or 2 additional pieces, depending upon the throw of a die, i.e., if Two or Four turns up, he gets nothing; if Five, Three or One turns up he gets one piece for each undeployed eight; and if Six turns up, he gets two.

(3) When the previous player, if any, has completed his move, the player whose turn it then is must begin by doing only one of four things:

(a) He may pass.

(b) He may allocate any number of his total of pieces in his reserved force to any frontier or frontiers, or transfer any number of pieces from one frontier to another, or withdraw any number of his pieces into his reserve from any frontier or frontiers; or any mixture of allocating, transferring and withdrawing from reserve to frontier.

(c) He may transfer pieces from amongst his income-earning resources to his reserved forces, or vice versa.

(d) He may make war against any one or more of the other Nations, as follows. The player picks up those of his dice deployed on the frontier opposite the Nation he wishes to attack, and rolls them. The attacked player similarly rolls his dice deployed opposite. Every player now has an opportunity to make war against any other player in the course of a battle. Each rolling of dice corresponds to a 'volley.' For each One thrown by either player, in any given engagement, the other relinquishes to Kitty one of the pieces he has rolled. Both players repeat this procedure,[4] volley after volley, using the remainder of dice previously rolled without adding to them, or subtracting from them except in accord with the rule, until one side's forces on the disputed frontier are eliminated. All engagements (battles) between all pairs of contestants must thus be fought to a finish. Immediately any one side's forces warfaring on any frontier have been eliminated,

> (i) the defeated player may, if he wishes, replace them with any number of his other deployed or reserved pieces,

[4] This procedure for war-making is derived from Ivan Driggs (1956). It is a simpler procedure than the following, which gives some advantage to attacking first, and some to being the defender, and which also provides an analogy to capitulation during battle—the attacker *alone* rolls his dice, removing to kitty one of the defender's deployed on that frontier for every One he [the attacker] manages to throw. Immediately after that, or after any subsequent volleys, either player has the option of surrendering to his enemy some or all of his embattled pieces. The number of pieces thus surrendered shall be deducted from any 'spoils of victory' owed to the attacker by the defender at the finish of the attacker's *next* move. After the attacker's first 'surprise' volley, subsequent volleys shall be simultaneous; but, while the attacker may remove to kitty one of his opponent's pieces for each One that he himself throws, the defender removes to kitty one of the attacker's pieces for each One and each Two that he himself throws.

provided such are not drawn from his income-earning resources; and under the same restriction he may re-allocate any other pieces;

(ii) the victor may claim from the defeated player one piece for every piece surviving in his victorious force as if in accordance with Rule 4 below; or, if and only if the vanquished has replaced his lost forces, the victor may reinforce his own forces on that frontier, using any pieces not currently engaged in warfare, and

(iii) first the defeated player, and then he shall have the choice of whether to renew the war. If either does so, all other players will again write out a chosen strategy, i.e., passing or making war. Turns in rolling pieces in any new wars that may thus ensue shall comply with the procedure previously mentioned in this rule, provided that all such turns shall be taken only after the completion of a round of turns by any couples already engaged in war.

Since all battles must be fought to a finish, warfare can end only by either the elimination of one or more players, or the vanquished player's not replacing his eliminated forces. The collection of 'victor's spoils' (see sec. ii, previous para.) formally completes such a conclusion of war. The original attacker then draws his 'income' for that round in accordance with Rule 2 above.

Receipt of income continues until kitty has been exhausted. This is the more likely to occur the greater the number of moves played. Immediately warfare has replenished kitty, receipt of income resumes.

(4) Immediately before completing his move by drawing his income (if any), a player who has pieces deployed along a frontier on the other side of which no pieces are deployed may, if he so wishes, claim from the player thus not defending, not more than one piece for each piece the claiming player has been deploying on the relevant frontier since immediately before the non-defending player's last previous move (whether in an ordinary round or in a special 'warfare' move). But

(a) if some third player (or further players) has had pieces deployed on another frontier not defended by the above-mentioned non-defending player since immediately before that non-defending player's last previous move as above, and if the total of pieces deployed on frontiers not defended by the said non-defending player exceeds the total of his deployed and undeployed pieces, each player thus eligible to claim pieces shall claim no more than a number equal to the total of the non-defending player's pieces multiplied by the pieces deployed by the given claimant on the relevant frontier, divided by the total of pieces deployed on frontiers not defended by the non-defending player. (This rule thus allows a Nation having forces on another frontier also not defended by the non-defending Nation to help the latter by *not* claiming for the forces he had previously deployed, thereby reducing the proportion that others can claim);

(b) all pieces handed over by the non-defending player shall in the first instance be drawn from his reserved forces and, if and when those are exhausted, one piece at a time from successive frontiers in an order the reverse of the ordinary order of play, provided that at least one of his pieces shall be left on each frontier for so long as an alternative claimed piece can be drawn from a frontier upon which he has more than one piece deployed; and no new claim against a non-defending player (thus created as a result of denuding a frontier by collection of claimed pieces) can be exacted until the next round—that is, until the conclusion of that next move of the new claimant's which is due at some time after the player whose claim initiated the collection has completed (see Rule 2) that move of which the said collection was the penultimate step;

(c) immediately after a non-defending player has satisfied all claims owed by him under this Rule 4, he may re-deploy any of his remaining pieces in any of the regular ways; and no such re-deployment shall count as an ordinary move or a move in a 'warfare' round of moves.

Pieces claimed and collected under Rule 4, or under Rule 3, Section 2, must be allocated to income-earning resources, but shall not begin to be counted towards the income-

earning groups of 8 pieces (see Rule 2) until after the collecting player's very next drawing of income due to him. The procedure under Rule 4 is intended as an analogue to 'spoils of victory' and the consequences of inferiority of numbers. We say that 'not more than' certain amounts of spoil may be collected, in order to allow victorious or predominant Nations to choose, e.g., between strategies of exacting indemnity from the defeated, and strategies of sparing him in order to "counter balance" other forces that might otherwise sweep the board. Rule 4 also allows an impoverished player something of 'the strategies of weakness': by facing a possible aggressor with the likelihood that some third or other player will be the beneficiary of an act of aggression, he can continue to have some effect. This completes the rules for "Balance of Power!"

Rules to extend the game

The game can be extended to reproduce some of the features of international politics supposed to accompany the introduction of *nuclear weapons* and of *inter-continental delivery-systems* by addition of the following rules.

(5) Any player who has in reserve at least 2 income-earning groups of 8 pieces may set aside, in addition, pieces to make up a 'research and development investment.' Pieces reserved for this purpose do *not* earn income. When this reserve has amounted to 6 pieces for one complete round, it gives the player a right to throw a die, once per move immediately after either passing, or transferring pieces, or completing the operations consequent upon making war (see Rule 3, especially para. c). As soon as he throws Six by this means, he may disperse his research and development investment and convert his 'conventional' forces on any given frontier into 'nuclear' forces by placing one piece (which must be drawn from his reserved forces) behind his forces deployed on that frontier. This piece cannot be rolled with the others in any warfare for so long as it is functioning as the 'nuclearizer,' though if isolated it can, as a 'conventional' unit, defend a frontier alone; nor can it earn income or contribute to 'research and development.' Its effect is to increase the destructive and self-defensive capabilities of the other forces on that frontier when war is on, as follows:

(a) during any war on that frontier, whether or not initiated by the 'nuclear' Nation, every One thrown by the nuclear Nation against the enemy's forces eliminates a number of them equal to the whole number nearest the square root of the pieces deployed by the enemy on that frontier; *or*, if occurring amongst a group (it may be the whole or any part of the pieces deployed by the nuclear Nation on the relevant frontier) expressly set aside as 'counter-resource weapons,' every One thrown eliminates automatically up to 8 pieces of the enemy's *income-earning* pieces. At the conclusion of every volley in which both sides have engaged, all the nuclear Nation's pieces deployed on that frontier shall be regarded, for purposes of elimination if any, as a single group; and before every volley any nuclear Nation entitled to 'fire' shall have the right to redivide the remainder into a counter-force and a counter-resource part. Each nuclear Nation shall be entitled to make *this* division *and* that mentioned under Rule 5 (d) below, *secretly*. But as soon as both he and his enemy (if also a nuclear Nation) have made their decisions, the firing shall become public. (In view of the general role of reserved forces in this game, they should be regarded as invulnerable to nuclear attack.)

(b) Any nuclear Nation *initiating* an attack shall have the right to count his first volley as a 'surprise attack,' i.e., his opponent shall not throw his dice in reply to it, but shall suffer any losses to his forces or resources, as under Rule 5a.

(c) Any nuclear Nation when firing, at any stage in a war, a volley which is *not* a surprise attack, shall have his defenses alerted, i.e., no matter how many Ones his opponent throws, any One, Three, or Five which he throws shall be safe from elimination during that volley; and any Six which he throws in that volley shall cancel any One which opponent (if also a nuclear Nation) throws in the 'counter-resource' part of *his* forces (if any) deployed on that frontier

i.e., throwing a Six with any of one's nuclear pieces deployed on a frontier *may* go to 'save' an income-earning unit of 8 pieces.

(d) If after one or more volleys, a nuclear Nation has eliminated all the forces deployed by his enemy on their common frontier, he may elect to turn his *counter-force* fire against forces deployed by that same enemy on *cny other* frontier or frontiers—secretly dividing his counter-force pieces before each volley into 'missions' severally directed against specified frontier-forces of his enemy. An enemy thus engaged may reply from such frontiers to the first as well as to other volleys.

(e) As soon as one Nation with the appropriate investment has thrown his Six and completed the move in which that was a step, any other player who, throughout one complete round, has been reserving, or thereafter throughout one complete round shall reserve 6 'research and development' pieces in addition to one set of 8 income-earning pieces may become a 'nuclear Nation,' and enjoy the same privileges as does the original discoverer under Rule 5 (a), (b), and (c) above.

It should be noticed that the 'deterrent effect' supposed to be exercised by nuclear weapons is represented by the *possibility* of a nuclear Nation's devoting his deployed forces to a counter-resource strategy, thus maximizing the chances of wiping out his opponent's reserves. It seems reasonable that the Nation should be allowed to keep secret his decision for or against such a strategy until the volleys are about to be fired. This permits a 'low confidence' strategy, i.e., his enemies do not know but what he might, if desperate, set off mutual destruction.

(6) Any 'nuclear' Nation whatsoever that has at the same time at least two groups of 8 pieces earning income may, by reserving 8 additional pieces (representing a missile program) for one complete round, throw a die as for the introduction of 'nuclear weapons' (see Rule 5), and upon throwing a Six acquire 'intercontinental nuclear forces' by grouping them in front of one 'nuclearizing' piece (see above, Rule 5), and if he decides to launch an attack may do so by allocating them various missions against any targets anywhere in the game. All first volleys are surprise attacks (except against other intercontinental nuclear forces—see below). The usual nuclear privileges of secrecy apply. So do the rules of scoring nuclear destruction, except in the following single case: all warfare between intercontinental nuclear forces reverts to the procedure of simultaneous volleys in which a turn-up of One eliminates one opponent (this since we shall assume that missiles are quick-firing solid-fuel jobs and that missile-launching sites are adequately dispersed). Once at least one nuclear Nation has become an 'intercontinental Nation,' other nuclear Nations can follow suit by the same procedure as used for becoming an n^{th}-country nuclear Nation, except that 8 non-income-earning pieces must be reserved instead of 6.

(7) If any intercontinental nuclear Nation wishes to, it may deploy on particular frontiers short-range nuclear forces or conventional forces, but those and all other forces at the beginning of any move must be *either* left as they are, *or* re-allocated, *or* used as they are to make war.

(8) Even though all its income-earning resources have been eliminated, any Nation can make war if it wishes to with any deployed forces it disposes of.

(9) Talking during the game and the making of agreements are permitted, but there is no rule of the game binding any player to observe an agreement he has made.

(10) Notwithstanding anything in the foregoing rules, the players at the beginning of a game may agree to the following restrictions upon deployment:

(a) Forces may be deployed on frontiers only if drawn *either* from forces already deployed *or* from pieces designated as 'intercontinental nuclear forces' *or* from a *general reserve* to the extent that has been maintained for at least one *ordinary* round. No pieces may be designated 'inter-continental nuclear forces' unless drawn from the general reserve.

(b) The general reserve may be replenished only in the course of an ordinary move, except that in the course of a move

made possible by some other player's declaration of war against a third player, the general reserve but not the deployed forces may be replenished on a one-for-one basis from forces deployed on other frontiers, or on a one-for-two basis from pieces kept for income-earning purposes (every surplus income-earning piece, thus used to buy the right of replenishing the reserve with a piece, shall be surrendered to kitty.)

(c) No piece, either while in the general reserve or for one round thereafter, can help to earn income.

(d) Pieces in the general reserve are the last of a player's pieces to be claimable as spoils of victory, unless he elects to surrender them earlier, in which case each piece in the general reserve is equal in value to any income-earning piece.

(e) Pieces in the general reserve may be attacked by any nuclear or intercontinental nuclear power in accordance with Rules 5(d) and 6, exactly as though they were pieces deployed on frontiers.

REFERENCES

Driggs, Ivan. A Monte Carlo model of Lanchester's Square Law. *Oper. Res.*, 1956, 4, 148-151.

Kaplan, M. A. *System and process in international politics*. New York: Wiley, 1957.

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It was a happy circumstance for the beginning of astronomy that in Kepler's time, the observations of Mars were not too exact. If they had been, Kepler would not have discovered that the curve described by the planet was an ellipse and he would have failed to discover the law of planetary motions. It was also fortunate that he chose to study the motion of Mars rather than the motion of the Moon, whose perturbations are much greater.

What was then due to good chance the method of successive approximations ought to do now. There are always those who complain that the scientific theories of economics or of sociology neglect certain details. That is their merit. We are first to get a general idea of a phenomenon, neglecting the details, considering them as perturbations; then to take in those details, first the more important and successively the less important.

PARETO, *General Sociology*

CRITIQUE AND COMMENT

The success of game theory in providing rigorous normative solutions to decision-making problems does not hide the fact that some realism has been sacrificed in the process. Proposed here is an interesting idea that an auxiliary concept—a common sense notion of commitment—can be used to bridge the gap between the normative formulation and typical behavioral processes in decision making. Perhaps several such concepts can be identified in the hope that a realistic and rounded framework for decision making can be developed without discarding the rigor and power of theoretical results already achieved. For other treatments of game theory in behavioral science, the reader is referred to the January, 1959 issue of *Behavioral Science* for an article by Anatol Rapoport entitled "Critiques of Game Theory," and to an article by T. C. Schelling named "Bargaining, Communication, and Limited War" in the *Journal of Conflict Resolution* (March, 1957).

COMMITMENT

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WITHOUT doubt, the Theory of Games has greatly stimulated and, on balance, accelerated the empirical study of human decision-making. This remarkable conceptual structure points the way toward formulating problems that previously appeared intractable. It has provided a precise language for describing phenomena of the decision-making process, and it has given rigorous normative solutions of decision problems against which actual behavior can be evaluated.

At the same time, Game Theory was not intended as a descriptive paradigm (except perhaps for large scale economic processes) and it does not adequately fill that purpose. Its success as a normative theory inevitably demands some sacrifice of behavioral realism. It is not our objective, however, to use the Theory of Games as a whipping boy. Rather, we hope to show that, if specific discrepancies between Game Theory assumptions and real life observations can be pinpointed, it may be possible to define auxiliary concepts that will bridge the gap. If these concepts are formulated with sufficient care, they may later be integrated, along with the Theory of Games, into a unified framework for decision-making behavior. At least in principle, this seems a highly desirable goal as against resignation to a normative, rational Game Theory on the one hand, and a distinct behavioral theory on the other.

One of the most daring and successful simplifying assumptions of Game Theory consists in the reduction of a decision-making problem to normalized form. The logic and procedure for this are fully described in various standard references (cf. Luce & Raiffa, 1954). Briefly, it consists in the assumption that all possible moves of, say, nature can be spelled out in advance as a set of "strategies." Then the decision-maker's problem is to select a sequence of countermoves, or a set of such sequences, that is good (in a technical sense) against the opponent's set.

This formulation is patently at variance with the typical behavioral process (except in a rather Pickwickian sense mentioned below). Most decisions that concern any issue of consequence are made a little bit at a time, sometimes, in fact, by imperceptible

degrees. The entire set of moves that nature may take is seldom fully explicated. Most important, perhaps, the decision is rarely made under a fixed degree of "risk" (cf. Luce & Raiffa, 1954) or under "uncertainty," but tends to occur within a sequence of increasing environmental certainty.

It will here be suggested that the common sense notion of commitment may be very useful for filling this particular gap between the normative and the behavioral aspects of decision-making. In its various usages, the term "commitment" refers respectively to the gravity of an overall decision; to the proportion of a total decision that has been made at any one time; and to the qualitative content of a decision—to the course of action one is "committed to." These ideas seem to embrace a large measure of what is missing in the Game Theory formulation of the decision-making process.

The present paper attempts to explore the meaning of the term "commitment" and to consider various possibilities for exact measurement. From the remark on different usages in the preceding paragraph, it is already evident that no single index will fully capture the meaning of this concept. However, we should be resigned to this outcome for any attempt to formalize a market place idea. What can be done, we believe, is to systematize the basis for various meanings and to indicate the range of their applicability. The final choice of an index, if such a choice seems desirable, may then be based upon external considerations. In particular, it will almost certainly depend upon the purposes to which we might wish to put an index of commitment.

OBSERVATIONS ON A DECISION-MAKING PROBLEM

To introduce some of the issues we feel are critical, we shall first pose a decision-making problem similar to one treated in the Game Theory literature (e.g., in Blackwell and Girshick, 1954). From this situation we attempt to draw certain morals that will become the basis for more extended discussion in a later section.

A man is engaged in a pistol duel, in which the adversaries march 10 paces from a center line, turn, and advance toward each other. Each man has two bullets and he may fire one round at any of his 10 forward steps. There are, accordingly, C_2^{10} response sequences in which both bullets are fired; C_1^{10} in which only one bullet is fired; and C_0^{10} sequences in which no bullet is fired. In all, there are $\frac{1}{2}(n^2 + n + 2)$ distinct response sequences from which to choose.

1. First, we observe that, while this can be treated as a typical game theory problem in normalized form, this approach is both sterile and unrealistic for psychological analysis.[1] From either the phenomenological or behavioral standpoints, the most interesting part of decision-making usually takes place on the spot. We could of course say that the state of the individual's synapses at the outset of the trial sequence constituted a grand strategy, since it predisposes him to a particular counter for any environmental input. This perfectly defensible position would, however, be the final resting place for our present venture. We find it more fruitful to treat a normalized strategy as the conjunction of a sequence of segmented strategies.

This does not mean, however, that we should simply break up the over-all decision into a series of "little decisions" and attempt to deal with these in isolation. The present position would be that segmental decisions are always made in context. An over-microscopic treatment of these segments may be as unrewarding as the global approach. Moreover, it may be very difficult in any real situation to determine the occurrence of little decisions. As anyone who has acquired a house, of a car, or a wife knows, the overt manifestations of decision-making may be little more than epiphenomena—and anticlimactic ones at that.

2. In the present example, there are ten potential decision points. If the duelist decides *not* to fire at the first step, then the number of *a priori* possible response se-

[1] A parallel would be an attempt to analyze Hamlet as a peculiarly happy choice among a large number of possible word sequences of approximately equal length.

quences is reduced from $\frac{1}{2}(n^2 + n + 2)$ to $\frac{1}{2}(n^2 - n + 2)$; if he fires, there are only n possible response sequences remaining. The greater reduction in the latter case corresponds to our intuitive feeling that the decision to fire entails a greater commitment than holding fire. The same is true for each step until the penultimate one. This in spite of the fact that the choice involved is formally a binary choice at each step (unless both rounds have been fired).

3. This extreme "lopsidedness" of decisions is in fact typical of many situations. It is tempting under these conditions to phrase the decision-making problem as "when shall the first round be fired?" In line with earlier remarks, however, we shall treat the problem as involving a sequence of "whether" decision segments. For one thing, this is the only way the problem can be formulated from a behavioristic standpoint.[2] Moreover, we assert that this is the psychologically (i.e., phenomenologically) valid form.

4. While the decision not to fire entails very little commitment, it does entail some. This inescapable minimum of commitment, which characterizes any decision process that takes time, will be referred to as "basal commitment."

Basal commitment is one aspect of a decision-making task that creates a pressure toward commitment. That is, if one is to be fully committed at some time in any case, he may as well make a congenial choice. Related to this is the fact that early commitment *per se* often seems advantageous, environmental circumstances being held constant. Both of these can be illustrated by the local politician who is identified with some candidate even if he sits on his hands and is in a position to request plums if he comes out for the right candidate soon enough.

5. Finally, we observe that the illustrative decision-making problem is unusual in several respects. For one thing, it seems like such an all engrossing episode in the life of our duelist that we are apt to forget that extra-duel considerations may influence his behavior. For most decision-making tasks, we canot afford thus to exclude the outside world. A related special feature of this task is that it has a clearly marked beginning and end. We merely note that no treatment of behavior sequences will be thoroughly adequate if it depends too much on this artificial property.

[2] Of course in the present situation we might not have 10 meaningful decision segments to observe. The duelist might fire both rounds before he had traversed the ten paces or his adversary might decide, correctly, to fire earlier.

COMMITMENT AS RESPONSE STRUCTURING

A natural and convenient index of the degree of commitment in a decision segment is the amount of information it carries about the final response choice. This becomes especially true if responses are considered in sequence as we have insisted they should be. In order to attach information to response sequences, however, it is necessary to assign initial probabilities on some grounds. Several possible grounds are examined below, but it is suggested that any probability assignment must be specifically tailored to the individual choice situation.

By all odds the simplest way of assigning probabilities to responses—if we can get away with it—is the device, used implicitly above, of considering all response sequences equally probable. The difficulties attending this approach are, of course, exactly those that have plagued the Principle of Insufficient Reason (cf. Luce & Raiffa, 1954). It would be the present writer's contention that the crucial difficulty lies not in determining whether there are any relevant bases for choice among alternatives but in fixing the field of alternatives.[3] That is, if we could agree that a decision-maker can have no reason for preferring alternative A to alternative B, then we can assign each a probability of $\frac{1}{2}$ If an inconsequential variant of A can be found, however, then this variant, say A', should be just as probable as A or B. This means that the probability of A is effectively raised to $\frac{2}{3}$, and so forth.

[3] Cf. the discussion of the paper presented by Braithwaite and Ayer in *Observation and interpretation*, edited by S. Körner (1957).

There are, of course, decision-making tasks in which the response alternatives are so clearly distinguished that no such subdivision is possible. This is comparable to our implicit use of the Principle of Insufficient Reason in assigning probabilities to heads or tails in a coin flip (the probabilities are not of course equal but our ignorance is entirely impartial and symmetric).

We shall take the position that where the response alternatives are in fact irreducible and are not essentially different, it may be quite appropriate to assume *a priori* uniform probability. Any departure from this (e.g., a known right-turn bias in a T-maze) can then be thought of as itself a sort of background commitment. The aspect of commitment that we are interested in, generally, will be the incremental commitment over and above this background.

We see immediately that this background commitment must be taken into account in any measurement situation. Although we took the duel more or less for granted in the previous section, we must recognize that anyone who finds himself back-to-back with an expert marksman on a cold November morning is already rather heavily committed. While we are not prepared to estimate the degree of commitment entailed by engaging in a duel, it seems quite possible that the principle of Indifference or Insufficient Reason provides a basis for such a measurement in less exotic decision problems.

The second broad approach to assigning probabilities to responses would be based on empirical observation either of the individual or of some sample of equivalent individuals. The necessary conditions for such observed probabilities to be valid are: first, that the situational context is representative; and, second, that the behavior is stationary, not undergoing systematic changes. If these conditions are fulfilled, there seem to be no other particularly acute problems in using empirical probabilities.

The third possible approach to the probability assignment problem is both the most interesting and the most precarious. This would be an approach that was based on the "payoff" values of various response alternatives. That is, we assume that, in some sense, a response sequence is probable to the degree that it leads to a high expectation of payoff under relevant environmental conditions.

TABLE 1

	E_1	E_2	E_3
R_1	6	4	3
R_2	4	5	2
R_3	3	5	6

Payoff values and probability

Various procedures for establishing an equivalence between payoff values and probability will be most readily discussed in connection with the hypothetical example of Table 1. This payoff matrix is in the customary form used in Game Theory, with minor changes in labeling. It is understood that the entire contents of the matrix are fully known to *S*.

The response probabilities might be based on the maximin solution or one of its variants suggested by Savage, Hurwicz, and others (cf. Milnor in Thrall, Coombs, & Davis, 1954). That is, the probabilities would be just the probabilities for a mixed strategy which would satisfy one of these criteria. All of these criteria appear to be so special, however, that they are unconvincing as a basis for an empirical paradigm. This weakness is further aggravated by the fact that none of them is completely satisfactory even on a priori grounds.

Less sophisticated, though perhaps more realistic psychologically, would be to assume a sort of "event matching" on the part of the decision-maker. Thus, we would set the probability of each response sequence proportional to the sum of probabilities of all *E*-states for which it is the optimal sequence. Our chief objections to this, as it happens, do not include the obvious point that probability matching is irrational.

One objection is linked specifically to our intention to use response probability as a basis for measuring commitment. It is apparent that the probability matching basis must bring in the *S*s information or belief (or ignorance) concerning the prob-

abilities of various E-states. Briefly (the point will be discussed later), we feel that this confuses response commitment with risk-taking.

A second objection is more technical. If a certain response sequence was only second best for all E-states, it would be accorded 0 probability by this approach. This seems distinctly counter-intuitive since such a response might well be the indicated one under any major departure from perfect information.

The procedure here advocated avoids both of these objections at some expense in computation. We consider the space of all "probability mixtures" of E-states, that is, all terms of the form $P(E_1), P(E_2), P(E_3) \ldots$ where each $P(E_i) \geqslant 0, \sum P(E_i) = 1$. This space corresponds to the totality of beliefs that S might hold about the relative probabilities of the various E-states. For each point in the space, there is an expected payoff value for each response based on the relative probabilities of all E-states at that point. It is then assumed that S selects each response with a probability corresponding to the proportion of the area in this "mixture space" for which the response is optimal. For the three responses in Table 1, the pseudo-probabilities are respectively 36.9%, 2.4%, 60.7%. The computational procedure is described in Appendix A.

We should not claim that this procedure of evaluation over a probability mixture space corresponds to any actual psychological process. It does, however, provide a completely unequivocal *a priori* measure that is perhaps not far removed from what *S*s go through in a less formal way. Moreover, the measure is not susceptible to distortion by arbitrary reshuffling of the response or E-state categories (assuming that payoff values are reasonably well-behaved functional values of these antecedents).

Given a suitable measure of the probability, prior to commitment, of a set of response sequences, there is a simple translation of any commitment into informational terms. If S is the set of initially relevant responses, then the "entropy" of the set is $-\sum_{i \in S} P(R_i) \log P(R_i)$, where $\sum P(R_i) = 1$ by assumption. Choice of some subset, T, of S reduces the entropy to $-\sum_{i \in T} P'(R_i) \log P'(R_i)$, where P' is the new probability within the set T (presumably, $P'(R_i) = P(R_i)/\sum_{i \in T} P(R_i)$). If a single response sequence is selected, then the latter entropy is zero and the commitment is equivalent to the total initial uncertainty. The actual computations will not be illustrated as they are readily available elsewhere (cf. Attneave, 1959).

This way of measuring commitment has several points in its favor. Where it is applicable—and this seems to be the case whenever probabilities can be defined—it provides a uniform index of commitment in standard units so that comparison is possible among various decision-making situations. In addition to this, the informational measure lends itself to a direct comparison of response commitment and the available environmental information on which that commitment is based. This comparison appears to be highly relevant to the topic of risk-taking, discussed at somewhat greater length below.

If we agree that commitment is in some way defined by the narrowing down of a set of response alternatives, the question arises as to how this constriction occurs. From our emphasis on sequences of responses, it is clear that part of the commitment process consists simply in performing certain root sequences of responses. Then the subset of possible sequences is confined to just those that begin with that particular root sequence. Curiously, this happens to correspond to our use of "commit" as a transitive verb (e.g., "commit a crime"). This is not, however, the most interesting or significant aspect of the concept. In a far more intriguing sense, commitment implies a projection from the root sequence—a redundancy of the behavioral "message." The next section will briefly explore three of the sources of such redundancy together with some of the more special indices of commitment suggested by each.

PROJECTED COMMITMENT

There are, of course, many circumstances which seem to compel particular actions, and

any of these can be thought of as a source of commitment. There are, for example, legal, ethical, social, and rational grounds for behavior. We shall not find it necessary to examine all of these explicitly since, for present purposes, the underlying formula is always the same. As stated above, it is assumed that commitment invariably consists in the implication of a root sequence of behavior for the total behavioral sequence in which it is imbedded.

There are, however, differences in the physical processes that embody these constraints. In the present section, we shall consider three such mediating processes: interresponse restrictions; environmental inertia; and, root-sequel interaction effects. It cannot be demonstrated that these categories are non-overlapping or exhaustive but they do seem to permit classification of a wide variety of cases.

Commitment as response restriction

In this section, we consider the reduction in opportunity potential that may come about due to the effects of a given response on the ability to perform other responses. A preliminary analysis is required of the response apparatus and of some of the constraints on response output.

We consider the response apparatus of any organism to consist of a set of action units that can be activated in various combinations. We shall usually be concerned with response sequences by which we shall mean a sequence of subsets of action units. More precisely, we understand behavioral time to be divided up into "epochs" by some temporal markers. During any epoch, a segment of behavior is defined by the activation or non-activation of each action unit. A response sequence, finally, is completely described in terms of a succession of such segments.

Response exclusion will refer to the tendency for one action unit, or subset of action units, to be incompatible with the simultaneous activation of other action units. This incompatibility may arise from psychological, physiological, or strictly physical factors. Specific exclusions are indicated by listing the subsets of action units that can occur during a given epoch.

Response preclusion is similarly the tendency for response activation during one epoch to interfere with activation of specific subsets during later epochs. The phenomena may be due to any of the causes mentioned for exclusion.

Epoch specificity will refer to the conditions under which certain action units may be ruled out during particular epochs regardless of other aspects of the response segment. It may happen, for example, that necessary manipulanda are not always present, etc.

In addition to these "hard" types of response constraint, there may be many other constraints that are not so easily specified in these terms. As an example, it might happen that there were no specific exclusions within a given task, but rather a general energy limitation that made the performance of response segments containing more than, say, 10 actions very unlikely. Going further, there may be "brokerage costs" attaching to response unit activation or deactivation which cannot be readily reduced to any specific relationships among responses. Our general position will be that such costs always imply constraints in a broader context, but we shall avoid the issue here by avoiding problems and examples that depend on soft costs.

A distinctive aspect of commitment as mediated by response constraints is that it is not necessarily tied to any particular set of environmental circumstances. That is, if a given root sequence of responses has been activated, we are not particularly concerned with the objectives of this root sequence. We are concerned rather with the consequences of this response output. Because of this emphasis, indices based on response constraint are particularly appropriate for a "running" measure of commitment.

The importance of this stems from observation number 5 attached to the initial example. It was noted that the situation in which decision-making can be related to a single objective and in which the response sequences are of fixed length are atypical. Using the present basis for measuring commitment, it is not necessary to assume a specific terminal epoch for the response

sequence. Instead we can integrate or total the reduction in response flexibility for as long as that reduction is discernible.

To illustrate this application, consider an organism with a response repertoire of three units: *a*, *b*, *c*. Activation of *a* rules out *b* for two subsequent units and rules out *c* for one subsequent unit. If there are no other inter-response restrictions, then there are, initially, eight possible segmental actions—i.e., no response or any subset of $\{a, b, c\}$. Activation of *a* cuts this down to only *a* or *o* in the following epoch, a two-bit decrease in entropy; the decrease in entropy in the following epoch is one bit (that is, *o*, *a*, *c*, or *ac* may then be used). We may if we like total these entropy decreases and say that the activation of *a* causes a total commitment of three "epoch-bits" before its effect is dissipated. This index can readily be extended to cover other forms of constraint and other bases for measuring response probability (here assumed uniform).

Environmental inertia

A second major sense of commitment that we shall investigate concerns the use of the term to refer to irreversible effects induced in the environment of the decision maker. In this situation, the response pool may remain intact after each response (unlike the response-constraint definition), and the effects of successive responses may be strictly additive or superpositional (unlike the following sense of commitment), but the activation of any response changes the environment once and for all. We propose to illustrate this situation with a very simple example and then to indicate the way in which the basic notions may apply to more general situations.

Consider the two "mazes" in Figure 1. They are identical in that each choice in both mazes is superficially a binary choice, and that retracing is prohibited in both. At the same time, it is clear that the "tree" maze, 1a, entails a deeper commitment, from almost any standpoint, than the "diamond" maze. In a sense, the "tree" maze is doubly irreversible while the diamond maze is partially reversible along the lateral axis.

Suppose we are concerned only with final outcomes—that is, with the last point that

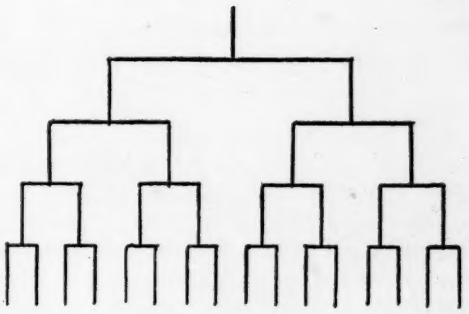

FIG. 1a. Tree Maze.

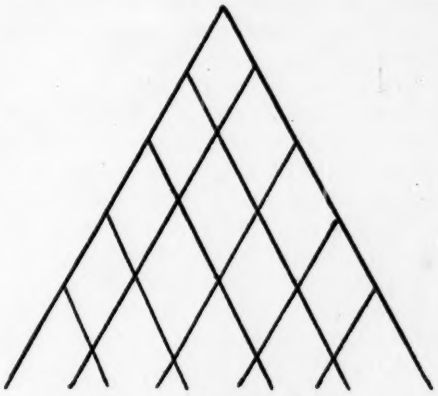

FIG. 1b. Diamond Maze.

is reached—as in most laboratory maze problems. Then, clearly, each choice in the tree maze cuts the number of potential terminal points in half: each choice in the diamond maze eliminates only a single terminal point. Hence the information in a tree maze choice is uniformly one bit, while that in a diamond maze choice increases from $\log_2 \frac{k}{k-1}$ bits for the first (of k) choices to one bit for the final choice.

In the real life analogs of such mazes, however, (cf. Simon, 1957) there is no reason for making a distinction between terminal points and other points in the maze. There may be payoffs at any point, and elimination of some subset of points automatically rules out certain potential payoffs. We are concerned not just with a set of terminal points but with an "opportunity cone"—a set of points that lie within potential access of the decision-maker. Looked at in this way, the tree maze decisions still carry exactly one bit of information for any maze of finite length. In the diamond maze, a decision at

step j ($o < j \leqslant k$) eliminates $k-j$ possible lattice points out of a possible $\binom{k-j}{2}$ such points.

In a 5-step diamond maze, for example, there are originally 15 possible lattice points (including the starting point). The first decision entails the selection of a subset of 10 of these points and discards of four points. The second decision selects six lattice points and discards three; the third choice selects three points and discards two; and, the final choice selects one point and discards one. Thus the total information carried by these choices in the diamond maze is $\log_2 \frac{14}{10} \cdot \frac{9}{6} \cdot \frac{5}{3} \cdot \frac{2}{1} = 2.8$ bits, as opposed to four bits in the tree maze.

To establish the relation between those mazes and real life decisions, we think of the x-axis as a time line; the y-axis continues to represent some environmental dimension. Then the diamond maze describes choice situations in which the actions taken are reversible from a purely physical standpoint. All we lose by an incorrect choice is a certain amount of time to enjoy the preferred E-state or to progress to even more desirable states. The tree maze, on the other hand, represents choices on irreversible environmental manipulations. Observe that time is important in such mazes primarily because it prevents us from retracing a path. We are reminded of Robert Frost's lines (1921):

"Two roads diverged in a yellow wood,
And sorry I could not travel both
And be one traveler, long I stood
And looked down one as far as I could
To where it bent in the undergrowth;"

It is a small step to carry this general scheme to a higher dimensional space, with time still occupying a central position. In doing this, we recognize that the form of the maze—tree, diamond, or some variant thereof—will depend upon the environment itself but also upon the response repertoire of the organism concerned. The notion of an "opportunity cone" is still entirely apposite for higher dimensional spaces.

Finally, we observe that all decisions in this section have been considered to be binary and symmetric. This assumption makes the situation easier to discuss but all the considerations on response probabilities, etc., of earlier sections might apply here. In particular, the "basal commitment" as defined for the illustrative mazes is a uniform function of time for the tree mazes and increases slowly with time in the diamond maze.[4] Of course, with symmetric probabilities, the basal commitment is indistinguishable from the over-all commitment.

Outcome-mediated commitment

The third sense of the term commitment to be investigated is related to specific response objectives. It hinges upon the fact that, in life, a particular root sequence of responses is rarely good on an absolute basis. Its value will, in general, depend upon both the succeeding events (or events that are manifest at a later time) and the terminal sequence that is employed. Thus, a given root sequence may "make sense" only if it can be followed by a specific terminal sequence, which is in turn indicated by later environmental conditions. When we say that we are committed to a certain course of action, we mean, frequently, that earlier actions indicate this terminal (or total) action sequence over a wide range of subsequently manifest E-states.

It is often true of "rigid" behavior that terminal sequences are based on root sequences even when later conditions are contraindicative. This corresponds to the idiomatic expression "throwing good money after bad." We may, for example, continue along a short-cut road after it has run out of macadam even though the total sequence of doubling back to the main highway might be considerably more rational. From a psychological standpoint, this aspect of commitment offers an obverse to the response-mediated interpretation. It is a commitment to a restricted subset of action sequences rather than a commitment away from the remainder set.

These general observations can be most readily exemplified by means of a simple

[4] If the maze is of finite length. An adequate treatment of the non-finite case would unduly expand the present paper. It is intended to suggest an approach to this problem in a separate publication.

hypothetical problem which will also suggest possible indices. Suppose, as in Table 2, that an S is presented with a decision-making problem in which he has two successive binary choices: A vs. a and B vs. b. The payoff depends on a play of nature that is presumably concealed from S until one or both of his choices are made.

Looking at Table 2, it is clear that S will only make the inital choice A if he expects to follow it with the choice B. As long as he is quite unsure of his second choice, or thinks there is a strong chance that he will later choose b, he will do best to select a. Thus, we can say that the choice A entails a stronger commitment than the choice a; we can also say that the choice A implies a commitment to the total sequence AB. If real-life impressions are a safe guide, Ss may choose the terminal response B to succeed A even if the E-state E_1 is strongly suspected.

This notion of commitment is closely related to our intuitive ideas of "risk" but differs from the latter in one important respect. While we ordinarily think of risk as being a joint function of the stakes involved and of the outcome uncertainty, the present notion of commitment is strictly independent of environmental information. The commitment implied by a given root sequence is just as great when it is based on absolute certainty as when it is associated with complete ignorance of the relevant conditions. In point of fact, many of our deepest commitments are made on the basis of utter certainty (which may be wholly chimerical).

TABLE 2

	E_1	E_2
ab	3	3
aB	2	2
Ab	1	1
AB	0	4

Source	df	SS
E-states	1	2
Aa	1	2
Bb	1	0
AaxBb	1	4
Residual	3	4

Hence, commitment in the present sense is cognate with the interaction effects (in the analysis of variance usage) between root sequences and terminal sequences as they affect outcome values.[5] In the specific case, the commitment involved in the choice of a given root sequence is very slight if it is an appropriate choice for any subsequent pairing with a terminal sequence. The commitment is heavy if a root sequence produces high payoff only when coupled with a particular terminal sequence: moreover, the commitment *to* that terminal sequence is also great.

The obvious index for measuring outcome-mediated commitment in a given decision situation would be the variance associated with the interaction between root sequences and tail sequences or some derivative, such as an intra-class correlation (cf. Haggard, 1958). For the present example, this interaction mean square is clearly large as compared with both the first order and residual mean squares.

SOME RELATED CONCEPTS

We have attempted in the foregoing section to maintain the position that the term commitment should be reserved for the purpose of characterizing the actual nature of the decision-maker's "play" against the environment. At the same time, we are aware that any commitment has certain consequences (resulting from nature's counterplay) that are intimately associated with the decision and with the meaning of the term commitment. The chief reason for maintaining a distinction—if it is possible to do so—is that metrically equivalent commitments may have markedly different consequences in different environments or at different times. We shall now briefly examine some general properties of these consequences.

Potential decrement

If the environment is essentially static, there is no "rational" reason for making a

[5] Here, and later in this paper, we use variance terminology in a broad sense. The actual measure of variability utilized would depend on distributions and other considerations.

commitment. There was, for example, no reason for the suitors in *The Merchant of Venice* ever to make a choice, providing that they could count on Portia's continuing hospitality and nubility. It appears to be true, in general, that commitment entails the exchange of potential opportunity for some degree of realized payoff.

One question that is raised is a semantic one, namely, is it possible to have a potential opportunity that is greater than the statistical expectation of actual payoff? We suggest that it is, in the same sense that the potential energy in a gallon of fuel is greater than can be exploited as power in any existing engine. The point is that, if there is some cosmic law of "conservation of opportunity," it obtains in a sense that is irrelevant to the interests of the individual decision-maker.

As a rule, however, there is some degree of basal commitment so that it is not possible to keep opportunity potential on ice. As we have previously suggested, it is also advantageous, in many cases, to make a commitment as early as possible.[6] Hence, the strategy of commitment consists in keeping it at a level which matches the degree of relevant environmental information. This is complicated in real behavior, however, by psychological factors: impatience and need for "closure," on the one hand, and superstitious resistance to being personally responsible for an outcome on the other.

Risk

The notion of risk, unlike that of commitment, appears to require consideration of the amount of environmental information on which the information is based. Thus the risk attaching to a given decision may even decrease if increasing commitment is based on proportionately increasing environmental information. We might, in fact, find it useful to define risk in terms of the ratio between information and commitment. In doing this, however, we should want a measure of commitment which took into account the relative utilities of various outcomes.

A more direct measure is suggested by the consideration that any set of probabilities for the relevant E-states, and any corresponding choice of response actions (or statistical mixture of responses), jointly defines a payoff variance. In general, this payoff variance will be greatest if the uncertainty of E-states is maximum (i.e., for uniform probability) and will decrease as environmental information is increased. So far, this seems in accordance with intuitive notions of risk.

It is further true, however, that any reduction in the pool of available response actions will tend to increase the outcome variance. Broadly speaking, this decrease is a function of the fact that the variance of the mean of a set of values varies inversely with the number of cases. It may of course be possible to select subsets of responses for which the payoff variance is decreased, but this simply corresponds to a conservative type of commitment.

These remarks do not by any means dispose of the problem of measuring risk. In particular, it might be objected that risk should be measured in terms of maxima or minima of payoff values rather than in terms of a random mixture. Such issues, however, are not germane to our major purpose which has been to indicate that risk can be given both a substantive and a metric definition that is different from the meaning attached to commitment.

Closure

Finally, there is a strong family relationship between commitment and the notion of "closure." To emphasize this relationship, we shall define the latter as "insulation from information." That is, we assert that much of the intuitive meaning that attaches to "closure" revolves around the fact that a course of action (or a sentiment) is invulnerable to further informational input.

The term "closure" tends to be used in reference to situations in which no clearly-marked action is defined. However, the general notion can be used, and measured, in decision-making problems. We shall merely define closure in this case as the

[6] Some care must be taken in specifying what is meant by this statement. Note that it cannot be taken to mean that responses should be made as early as possible unless we have defined a null response. It is also assumed that, in comparing early commitments with delayed commitments, they are evaluated under the same final E-states.

TABLE 3

	E_1 (.1)	E_2(.2)	E_3(.3)	E_4(.4)	
ab	7	5	3	4	4.2
aB	3	8	4	2	3.9
Ab	4	5	6	4	4.8
AB	5	4	3	7	5.0

reduction in value of information as a consequence of commitment.

In the example of Table 3, the expected payoff value, with no information other than probabilities given in parentheses, is simply the expected value for the best overall sequence, AB. This value is 5.0 "utes" (with apologies). With full information, and complete freedom of choice for R_i, the expected value (over all E-states) is 6.9 utes, an increase of 1.9. The total entropy in the four E-states with indicated probabilities is 1.84 bits. Hence, information is worth $\frac{1.9}{1.84}$ utes per bit.

If now S chooses the pair Ab and AB (by selecting the initial unit A), his expectation for no information remains at 5.0, but his expectation under full information is decreased to 6.1 so that information is worth only $\frac{1.1}{1.84}$ utes per bit. Final choice of Ab or AB completely negates the value of subsequent information. It should be observed that the selection of ab and aB, through initial choice of the action unit a, would necessarily be based on initial information indicating higher probabilities for E_1 and E_2 than those given in Table 3. If these new probabilities are known, the calculations proceed in the same way as those shown.

The foregoing remarks fall far short of a complete explication of the meaning of the three concepts that were to be compared with commitment. It does appear though that there are pronounced, and perhaps useful, differences among these concepts, and between each of them and commitment. Decrement in potential or opportunity seems to be an inevitable concomitant of commitment; at the same time, the former term is broader and perhaps more descriptive of environmental processes than of decision-making behavior. Risk taking is more closely tied to possible outcome values and, in addition, is specifically related to the degree of certainty or uncertainty concerning the environmental state. Closure, as we have defined it, is a more cognitive notion, closely associated with commitment, but not coextensive with it.

We have undertaken this digression from the investigation of commitment proper for several reasons. First, by explicitly defining and excluding these related concepts, we have perhaps reassured the reader that our conception of commitment, while still very broad, does not extend indefinitely in all directions. A more positive objective, however, is to show that commitment may be only part of a family or continuum of constructs lying between Game Theory and empirical behavior. The fact that several such concepts can be identified raises the hope that a realistic and rounded framework for decision-making can be developed without discarding the rigor and power of theoretical results already achieved.

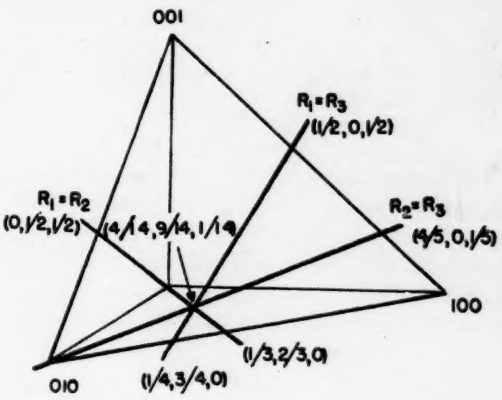

FIG. 2a.

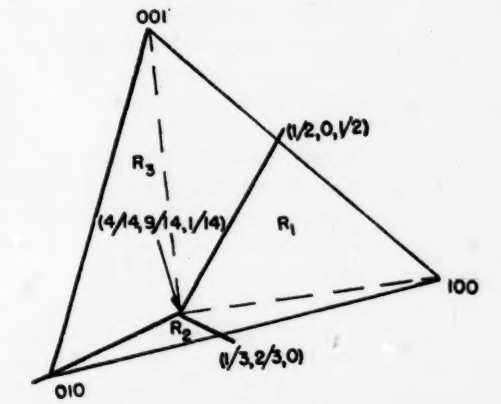

FIG. 2b.

APPENDIX A

All the admissable probability mixtures of the *E*-states 1, 2, and 3 are represented on the triangle connecting the points (100), (010), (001) (see figure 2). The problem is to compute the proportion of this space over which each of the R_i is optimal.

The first step is to determine the lines at which the expected payoffs for each pair of R_i are equal. For R_1, R_2; R_1, R_3; R_2, R_3, these lines are respectively $2\zeta_i - \zeta_{2+3} = 0$; $3\zeta_1 - \zeta_2 - 3\zeta_3 = 0$; and $\zeta_1 - 4\zeta_3 = 0$. It can be determined by inspection which R_i dominates the area on either side of any line and the total region for each R_i is the area over which it dominates both other R_i.

The next step is to determine where the equal value lines intersect each other and intersect with the edges of the probability mixture space. In the present problem (not in general), all three lines intersect at the point $(\frac{4}{14}, \frac{9}{14}, \frac{1}{14})$. The three polygons determined in this way are shown in Figure 2b.

The final step is to determine the area of each polygon from the expression for its vertices. For a triangle with vertices $(x_1y_1z_1)$, $(x_2y_2z_2)$, $(x_3y_3z_3)$, the square of the area is obtained from the sum of squares of the projected areas by the formula:

$$4a^2 = \begin{vmatrix} x_1 & y_1 & 1 \\ x_2 & y_2 & 1 \\ x_3 & y_3 & 1 \end{vmatrix}^2 + \begin{vmatrix} x_1 & z_1 & 1 \\ x_2 & z_2 & 1 \\ x_3 & z_3 & 1 \end{vmatrix}^2 + \begin{vmatrix} y_1 & z_1 & 1 \\ y_2 & z_2 & 1 \\ y_3 & z_3 & 1 \end{vmatrix}^2,$$

In the present case, separate calculations of the three determinants is unnecessary because of the uniform 45° projection.

For polygons of more than three sides it is necessary to compute areas separately for the included triangles and add them. The area for the total mixture space is of course computed in the same way.

REFERENCES

Attneave, F. *Contributions of information theory to psychology*. New York: Henry Holt, 1959.

Blackwell, D. & Girshick, M. A. *Theory of games and statistical decisions*. New York: Wiley, 1954.

Frost, Robert. The road not taken. From *Mountain interval*. New York: Henry Holt, 1921.

Haggard, E. A. *Interclass correlation and the analysis of variance*. New York: The Dryden Press, 1958.

Körner, S. (Ed). *Observation and interpretation, (First session)*. New York: Academic Press Inc., 1957.

Luce, R. D. & Raiffa, H. *Games and decisions*. New York: Wiley, 1954.

McCrea, W. H. *Analytic geometry of three dimensions*. New York: Interscience Publishers Inc., 1957.

Simon, H. A. Rational choice and the structure of the environment. In *Models of man*. New York: Wiley, 1957.

Thrall, R. M., Coombs, C. H., & Davis, R. L. (Eds). *Decision processes*. New York: Wiley 1954.

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Or if we must play the theological game, let us never forget that it is a game. Religion, it seems to me, can survive only as a consciously accepted system of make-believe. People will accept certain theological statements about life and the world, will elect to perform certain rites and to follow certain rules of conduct, not because they imagine the statements to be divinely dictated, but simply because they have discovered experimentally that to live in a certain ritual rhythm, under certain ethical restraints, and as if certain metaphysical doctrines were true, is to live nobly, with style. Every art has its conventions which every artist must accept. The greatest, the most important of the arts is living.

ALDOUS HUXLEY, *Texts and Pretexts*

COMPUTERS IN BEHAVIORAL SCIENCE

Computer Simulation of Concept Attainment

Carl I. Hovland and Earl B. Hunt, Yale University

The effectiveness of utilizing high speed computers for simulation of psychological processes is brilliantly demonstrated in the problem-solving model of Newell, Shaw, and Simon (1958). The process of concept attainment lends itself readily to similar analysis and simulation. Accordingly, the writers have been developing a series of computer models for simulation of human concept formation.

Our first computer simulation was based on the concept learning model of Hovland (1952). Here the subject (or machine) knows the dimensions which will be used, the number of values of each, and the number of dimensions and values required to define the concept. All possible conjunctive and disjunctive concepts generated from the known structure are stored. The instances presented during learning are used to confirm or infirm each possible answer. This provides an algorithmic procedure for solving a concept attainment problem as soon as sufficient information is made available to the machine.

As an example of the operation of this machine conceptualizer, consider a concept based on stimuli described by three dimensions with two values for each. The subject (or machine) is informed that the concept to be found involves two dimensions with one value on each. Thus, if the dimensions are color (black, red), size (large, small), and form (circle, triangle), the following "two dimensional concepts" are possible; large circles, large triangles, small circles, small triangles, large black objects, large red objects, small black objects, small red objects, black circles, red circles, black triangles, red triangles. Instances encountered during learning are of the form large black triangle, small black circle, etc., and are labeled as positive or negative exemplars. An informationally adequate set of instances is one which cuts the possible "common" two dimensional description of positive instances from the original twelve alternatives to the appropriate one. For instance, if one gives the positive instance small red circle and then the negative instances small red triangle and large red circle, the concept must be "small circles." When only one hypothesis consistent with the informational input remains, the computer prints out this concept.

The second model was developed by Hovland, Taylor, Shure, and Levy (1958) during the 1958 Social Science Research Council Institute on Simulation of Cognitive Processes. It was programmed for the RAND JOHNNIAC computer. This model had the added feature of simulating selective attention to certain aspects of the stimulus. For example, if an instance contains information characterized by a description "Three large red triangles placed symmetrically on a blue background," a given subject might only attend to the dimensions number, color, and background. If the concept should involve the size dimension, a subject cannot solve the problem until he begins to attend to size. Similarly, if it involves symmetry, he must attend to this dimension. In the model, provision was made for noticing additional dimensions as learning proceeded; a scanning arrangement was introduced whose transmissions could be changed during acquisition. Another modification was to drop the requirement that the exact structure of the concept be known in advance. Tentative answers were generated from the instances as presented instead of being selected from a predefined set as in the prior machine model.

The model involved selection of single dimensional concepts. The human subjects being simulated indicated that they noticed only certain characteristics of the stimulus and that they generated unidimensional concepts from these; for example, "The positive stimuli are all circles." These hypotheses were revised until the final concept was the correct one. In our experiment, this was "symmetrical figures." The computer

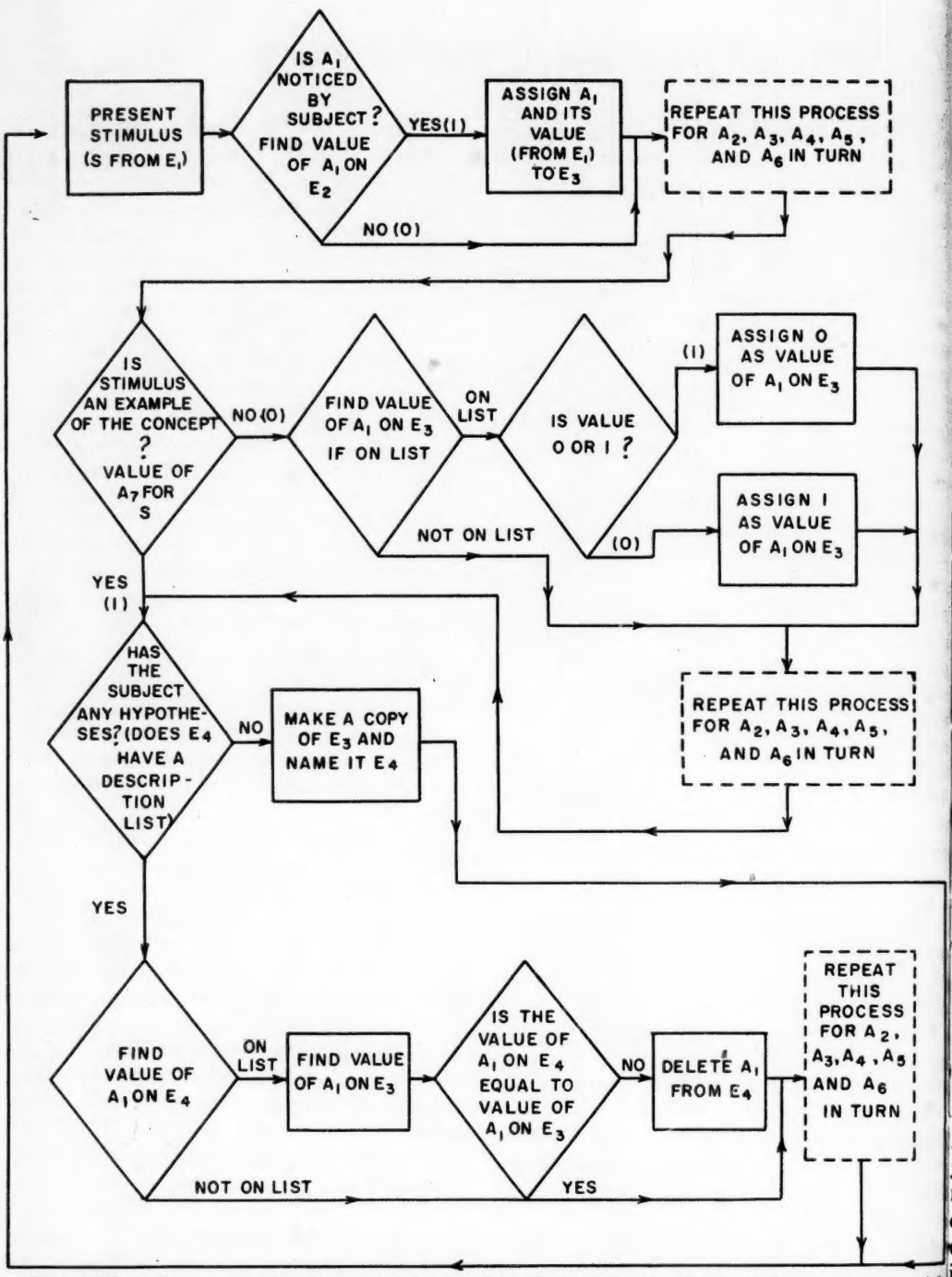
PRESENT STIMULUS (S FROM E_1)
IS A_1 NOTICED BY SUBJECT? FIND VALUE OF A_1 ON E_2
YES(1)
NO (0)
ASSIGN A_1 AND ITS VALUE (FROM E_1) TO E_3
REPEAT THIS PROCESS FOR A_2, A_3, A_4, A_5, AND A_6 IN TURN
IS STIMULUS AN EXAMPLE OF THE CONCEPT ? VALUE OF A_7 FOR S
NO(0)
FIND VALUE OF A_1 ON E_3 IF ON LIST
ON LIST
NOT ON LIST
IS VALUE 0 OR 1 ?
(1)
(0)
ASSIGN 0 AS VALUE OF A_1 ON E_3
ASSIGN 1 AS VALUE OF A_1 ON E_3
YES (1)
HAS THE SUBJECT ANY HYPOTHESES? (DOES E_4 HAVE A DESCRIPTION LIST)
NO
MAKE A COPY OF E_3 AND NAME IT E_4
REPEAT THIS PROCESS FOR A_2, A_3, A_4, A_5, AND A_6 IN TURN
YES
FIND VALUE OF A_1 ON E_4
ON LIST
FIND VALUE OF A_1 ON E_3
IS THE VALUE OF A_1 ON E_4 EQUAL TO VALUE OF A_1 ON E_3
NO
DELETE A_1 FROM E_4
REPEAT THIS PROCESS FOR A_2, A_3, A_4, A_5 AND A_6 IN TURN
NOT ON LIST
YES

FIG. 1. Flow diagram for simple concept former. List E1 is a stimulus list loaded into machine with all dimensions and values indicated. This is then screened (by list E2) to simulate the restriction of perceived dimensions. These lists generate a "perceived dimension" list, E3. Unidimensional hypotheses are then deduced from the first instance and subsequently narrowed down as successive stimulus inputs generate additional hypotheses to be reconciled. Finally, only the appropriate concept remains (if the dimension is perceived) or no appropriate concept remains (if the relevant dimension is not perceived). (From Hovland, et al., 1958.)

language used was IPL-IV, developed by Shaw and his colleagues. The flow diagram is given in Figure 1.

The behavior of this model still possesses a number of features which differ from the performance of human concept learners. Some of these are: (1) the machine provides for unlimited memory storage whereas humans may fail to attain the correct concept because they do not remember the information contained in earlier instances (cf. Cahill and Hovland, 1960), (2) the model can handle only conjunctive concepts of a relatively simple type although humans can solve multidimensional, relational, and perhaps disjunctive concepts (Hunt and Hovland, 1960), and (3) no provision is made for a preference for answers which utilize particularly "salient" dimensions.

Our current research is oriented toward investigating the operation of memory in concept attainment. Performance will be simulated under one set of conditions where no "cognitive strain" due to limited memory is involved. The requirement of memory for previously presented instances will then be superimposed. In one model, "images" of the previously presented instances will be retained. In another, only the results of tests applied to the information contained in these instances will be retained. These models will have the capability of operating on both conjunctive and relational concepts.

To aid in developing these more complex models, a supplementation of the IPL-V computing language is being developed. In the supplemented language, all the primitives of IPL-V are permissible, but in addition a format for setting up a hypothetical concept learning experiment (including specification of subject variables) and several new primitives are provided.

The format is essentially a "fill in" outline by which the characteristics of the subject, the stimuli, and the concept are specified. The experimenter defines what capabilities the "subject" has, both in processing and remembering information. Specification of the values on the description lists of one list structure determines the initial hierarchy of the subject's problem-solving responses. Specification of another list structure defines the subject's memorizing capability. In the stimulus list structure, the order of instances is prescribed and the instances themselves are described in terms of dimensions and values, as in the first model discussed. However, the same stimulus may have several descriptions, depending upon how a dimension is to be defined. Any number of such descriptions may be used in a single program but, of course, only one description may exist in the computation field at any one time. Thus it is possible to write a heuristic program which selects one of several alternative ways of describing a stimulus and then searches for categorizing rules within that description. The format also contains an answer and a definition section. These are essentially housekeeping procedures for providing an answer and for "signalling" the additional routines when they arrive at a branch point.

The concept learning primitives trade the flexibility of IPL-V for greater power in handling this particular type of problem. They are used in exactly the same manner as IPL-V primitives and the same "safety rules" apply. The difference between the new routines and the IPL primitives is that the new routines assume that their inputs have the specific form required by the appropriate format section. This permits rapid programming of a variety of complex searches, divisions, and alterations of list structures which would be tedious to write out in IPL-V. In the near future, we will report the results of experiments matching human behavior against that of the more complex models of memory and concept attainment which can be constructed with these programming tools.

REFERENCES

Cahill, H. E. & Hovland, C. I. The role of memory in the acquisition of concepts. *J. exp. Psychol.*, 1960, 59, 137–144.

Hovland, C. I. A "communication analysis" of concept learning. *Psychol. Rev.*, 1952, 59, 461–472.

Hovland, C. I., Taylor, D. W., Shure, G. H., & Levy, N. A simple concept former. *S.S.R.C. Summer Inst. Memorandum*, (mimeo), 1958.

Hunt, E. B. & Hovland, C. I. Order of consideration of different types of concepts. (*J. exp. Psychol.*, 1960, 59, 220–225.

Newell, A., Shaw, J. C., & Simon, H. A. Elements of a theory of human problem solving. *Psychol. Rev.*, 1958, 65, 151–166.

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COMPUTER PROGRAM ABSTRACTS

Detailed programs of computer program abstracts 19–23 have been deposited with the ADI Auxilliary Publications Project, Photoduplication Service, Library of Congress, Washington 25, D. C. A copy of the program desired may be secured by citing the document number and remitting the proper amount for photoprints, or 35mm microfilm. Advance payment is required. Make checks or money orders payable to: Chief, Photoduplication Service, Library of Congress.

Partial Item Analysis on the IBM 650, *Glen Walker, University of Washington* **(CPA 19)**

This program tallies the responses for as many as 80 items simultaneously, computes the percentage of the total responses which fall into each category for every item, omitting the "no responses" if that is desired, and punches out the answers giving the tallies for an item on one card and the percentages for the same item on another card. Computer time is about four seconds per subject. Cross comparisons between items can be obtained by sorting the cards on one column and tallying the other columns. No more than 9 categories per variable are allowed.

Order Document number 6280 and remit $1.25 for photoprints, or $1.25 for 35mm microfilm.

A General Correlation Program for the IBM 650 with Tapes, *Elliot M. Cramer, Biometrics Branch, National Institutes of Health.* **(CPA 20)**

This program computes intercorrelations, means, and standard deviations for an unlimited number of variables when there may be missing data points for some or all variables. The number of data points for any variable should be less than 10,000. The program is self-restoring so that any number of matrices can be processed. Several input and output formats are available and sequence checking of the input cards is optional. Index registers and at least three tape units are required. No time estimate is available but for 50 variables and 50 datapoints, the time was 20 minutes with no missing data and 30 minutes with missing data.

Order Document number 6281 and remit $2.50 for photoprints, or $1.75 for 35mm microfilm.

For a SOAP listing (with comments) of the program, order Document number 6282 and remit $5.00 for photoprints, or $2.25 for 35mm microfilm.

A Minimal IBM 650 Computer Program for Converting MMPI Raw scores to t-Scores, *Clayton E. Ladd, Indiana U. Medical Center, Indianapolis; John P. Dolch and Leonard D. Goodstein, State University of Iowa* **(CPA 21)**

The program, developed for a minimal IBM 650 computer, provides an efficient means of converting MMPI raw scores to t-scores and a convenient method of storing the data in an accessible and useable manner. Input cards (up to approximately 10,000 subjects of each sex) indicating a subject's name, sex, MMPI raw scores on three validity and nine clinical scales, and composite score from college entrance examinations are read, and the following operations performed at the rate of 100 cards per minute:

1. For the MMPI clinical scales using a K correction, the correct proportion of K is computed and added to the scale's raw score.

2. The raw scores (with K corrections when appropriate) of the clinical scales are converted, by a direct address procedure, to t-scores. This conversion depends, of course, on the sex of the subject.

3. The number of validity scales above specified cut-off points and the number of clinical scales above a t-score of 70 are counted separately for each subject.

4. The identifying information, the raw scores of the validity scales, the t-scores of the clinical scales, the indices of deviation on the validity and clinical scales, and the entrance test composite score for each subject are punched on an output card.

5. The test variables on the output cards are summated over individuals, and two final output cards indicate the group means by sexes.

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Univac Scientific Computer Program for Scaling of Psychological Inventories by the Method of Reciprocal Averages, *Frank B. Baker, University of Minnesota* **(CPA 22)**

The Method of Reciprocal Averages uses the a priori knowledge of the investigator as the basis for assigning item response weights by an iterative procedure. After each iteration, the sum of the absolute differences between successive sets of weights is found. If the sum is less than a convergence parameter, the procedure i

terminated. The resultant weighting scheme maximizes the internal consistency of the responses to the inventory.

The input is a facsimile of each subject's inventory, an a priori weighting scheme, and a set of parameter cards describing the problem. The input information is on 80-column punched cards which have 6 fields of 12 octal digits each and an identification field of 8 columns. The total of all item choices c must be less than 504, no item i can have more than 7 choices, missed items are punched zero and ignored. The maximum number of subjects s is 767. A further limitation is $cis \leq 8192$.

The output is the final weighting scheme, a weighted total score for each subject, and the number of persons selecting each item response.

A system whereby specified items can be eliminated from the analysis, without modifying the input data, is under development.

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An IBM 650 Program for Kendall's Tau, *Samuel L. Becker, State University of Iowa* **(CPA 23)**

Tau is a measure of the degree of correspondence between two sets of rankings with tied scores. This program computes up to 190 taus from a matrix of raw data. The number of variables can be 20. The number of observations times the number of variables can be 1200. The program is for the basic IBM 650.

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Regression Analysis, *K. K. Bailey, Murray Berg, D. C. McGowan, L. G. Painter, and E. J. Thompson, Standard Oil Company of California.* **(CPA 24)**

Description: The program performs a multiple regression and correlation analysis. The maximum size problem is 39 independent and one dependent variables. Back solutions are obtained to six significant figures. **Computer:** IBM 704, 4K or larger. **Programming Language:** SAP and FORTRAN II. **Approximate Running Time:** For a *maximum* size problem: FORTRAN Assembly-5 minutes; Input Editor Run-8 minutes; Regression Run-10 minutes. **Comments:** The input editor portion transforms the observed variables, V_i, into function variables $X_j = f(V_i)$, which are the actual regression variables. A FORTRAN program to compute all the function variables, X_j, used in a given set of problems, is compiled and becomes a part of the input editor program. Restrictions for the input editor are 1,000 observed variables, V_i, and 400 observations of these variables. The number of function variables is unlimited. **Availability:** Presently being used for production. A manual may be ordered from the American Institute of Chemical Engineers, 25 West 45th Street, New York 36, New York. Reprinted from *Chem. Eng. Prog., 1959, 55: 6, 134.*

Non-linear Estimation, *G. W. Booth and T. I. Peterson, IBM, White Plains, New York.* **(CPA 25)**

Description: The program treats the following statistical problem. Given a functional relation between a single dependent variable and a set of K independent variables and P parameters, and given data for N observations of the dependent variable and NK corresponding values of the independent variables, provide: (1) estimates for the parameters by an iterative least squares procedure, and (2) information to assess the worth of the estimated parameters. The functional relation may be linear and/or nonlinear in the parameters as well as the independent variables, and may be explicit or implicit, the necessary condition being that it admit of numerical evaluation. The program may be used for the case of more than one dependent variable. It is based on methodology developed by G. E. P. Box, Director of the Statistical Techniques Research Group, Department of Mathematics, Princeton University.

Computer: IBM 704, 8K core. **Program language:** SAP. **Running time:** Computational time is dependent on many factors. A moderate problem (N = 25, K = 3, P = 5) requires about one minute per iteration for an explicit function, and approximately three times as long for a function requiring the solution of differential equations. **Comments:** The program has been used in scientific and business problems of various types. Information provided by the program includes: (1) least squares estimates of the parameters, and (2) data for: (a) analysis of the residuals, (b) analysis of the correlation between estimates of the parameters, (c) a check that the least squares estimates truly correspond to a local minimum, (d) evaluation of the linearity of the functional relation with respect to the parameters in a local region, (e) analysis to determine the local nature of the minimum, and (f) approximate confidence regions for the estimated parameters. **Availability:** The manual for this program can be ordered from the American Institute of Chemical Engineers, 25 West 45th Street, New York 36, New York. Reprinted from *Chem. Eng. Prog., 1959, 55: 7, 90.*

Curve Fitting, *R. A. Eimer, Socony Mobil Oil Company, Paulsboro, New Jersey.* **(CPA 26)**

Description: This routine fits a fifth-degree polynomial to ordinate values given at 11 equally spaced abscissa values. The coefficients in the polynomial

$$Y = A_5 x^5 + A_4 x^4 + A_3 x^3 + A_2 x^2 + A_1 x + A.$$

are computed making use of orthogonal polynomials. The sum of the squared residuals is computed for fitting with a constant term and for each additional power up to 5. This gives an idea of how the quality of the fit is improved by increasing the degree of the polynomial. Provision is made to compute polynomials of less than fifth degree. **Computer:** Burroughs 205 (Datatron). **Program language:** Machine language. **Running Time:** About 15 seconds. **Availability:** A manual will be prepared for sale by the American Institute of Chemical Engineers, 25 West 45th Street, New York 36, N. Y. Reprinted from *Chem. Eng. Prog., 1959, 55: 8, 78.*

TRAP—TAPE Regression Analysis Program (IBM 650-Tape). RAPA—Regression Analysis Program (Augmented IBM 650). RAP—Regression Analysis Program (Basic IBM 650), *J. E. Nichols, Shell Oil Company, Houston Research Laboratory, Houston 1, Texas.* **(CPA 27)**

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The programs provide for the entry of thirty-two variables and up to 999 observations. Nine dependent variables can be correlated in one pass in the TRAP and RAPA programs while eight is the maximum number in the RAP program. The regression equation to be fitted may contain a maximum of twenty-six terms including dependent variables. Each term may be the product of up to five transformed variables all raised to various powers ranging from 0.1 to 9.9. Variable transformations are done by means of codes and constants. The programs provide for thirty-two constants and thirty-two codes. Data are entered as positive and/or negative, four-digit floating decimal numbers. The automatic floating decimal device is used by TRAP and RAPA programs, and RAP uses the programmed floating decimal arithmetic SIR (Symbolic Interpretive Routine).

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Nonlinear Regression by Criterion of Least Squares, *G. G. Bejarano, R. M. Baer, and L. Tornheim, Mathematical Services Group, California Research Corporation, Richmond, California* **(CPA 28)**

Description: This program determines the least squares fit for a large number of data points to an equation which is nonlinear in the parameters. The equation may also be nonlinear in the varibles. Examples of such nonlinear equations are:

$$Y = \log\left[\frac{a_1 + X_1^{a_2}(100 - X_1)^{a_3}}{a_1 + X_2^{a_2}(100 - X_2)^{a_3}}\right]$$

$$Y = aX_1 + b_1 e^{b_2 X_2}$$

A modified Gaussian procedure is used. Back-solutions and statistical data are obtained as

well as the constants (a_i, b_i) in the equation. **Computer:** Datatron 205, 4000 words storage, card input and output, and floating point unit. **Program language:** Datatron 205 machine code. **Running time:** Depends on the form of the equation, the number of parameters and the number of observations. However, most solutions require from 10 to 30 minutes. **Comments:** A subprogram must be written for each nonlinear equation and certain of its derivatives. The program has been used successfully on a wide variety of complex equations. **Availability:** A manual will be prepared by the American Institute of Chemical Engineers, 25 W. 45th Street, New York 36, New York, if sufficient interest develops. Reprinted from *Chem. Eng. Prog., 1960, 56: 3, 86.*

Growth Curves, *L. H. Krone, Applied Mathematics Section, Monsanto Chemical Company, St. Louis 66, Missouri.* **(CPA 29)**

Description: The program will fit a variety of growth curves, using the method of least squares to obtain estimates of the regression coefficients. Extrapolations are made for an arbitrary number of future years and confidence limits are obtained for the extrapolations.

The curves fitted by the program are:

Linear

$$y = A + Bx$$

Quadratic

$$y = A + Bx + Cx^2$$

Semi-log

$$\log y = A + Bx$$

Quadratic in logarithms

$$\log y = A + Bx + Cx^2$$

Logistic

$$y = KA^{Bx}$$

Gompertz

$$y = K/(1 + \exp(A + Bx))$$

The equation or equations desired in a particular case are indicated on a control card which also contains convergence criteria or iteration limit.

Computer: IBM 704, 8K core, 1 tape (optional-output). **Program language:** Fortran II. **Running time:** This is a function of the number of data points and the amount of extrapolation requested. A typical case of 20 data points and a 10-year extrapolation would run about a minute, including on-line output. **Availability:** A program manual can be made available for publication by the American Institute of Chemical Engineers, 25 West 45th St., New York 36, N. Y., should sufficient interest develop. Reprinted from *Chem. Eng. Prog., 1959, 55: 11, 92.*

Curve Plotting Routine, *H. D. Eddy, Standard Oil Company of California, San Francisco, California* **(CPA 30)**

Description: This program reads a set of X and Y data points (a maximum of 500 pairs), orders them by decreasing values of Y, and prints out a graph of either 50 x 100 or 100 x 100 divisions. Any grid character and plotting character may be used. **Computer:** IBM 704, 4K or larger. **Program language:** Fortran. **Running time:** Time for each problem after the binary deck has been loaded is from six to fifteen seconds, depending upon the number of data points and the size of the output sheet called for. **Comments:** This program is self-sufficient and can read data from cards or from a tape previously prepared from another 704 problem. It may be used to plot the results of the Tarner calculation. **Availability:** This program is presently being used for production, and a complete program write-up, less flow diagram, is available at this time. Flow diagrams can be prepared if this program is accepted for publication by the American Institute of Chemical Engineers, 25 West 45th St., New York 36, N. Y. Reprinted from *Chem. Eng. Prog., 1959, 55: 9, 86.*

Response Surface Evaluation, *Allan W. Dickinson, Monsanto Chemical Company, Applied Mathematics Section, St. Louis, Missouri* **(CPA 31)**

Description: The program evaluates a first or second degree polynomial in two, three or four independent variables, and prints the results in tabular form. Tape output is optional. **Computer:** IBM 704, 4K core, 1 tape (optional). **Program language:** Fortran II. **Comments:** Up to fifteen items may be printed in a single line of the matrix. The largest number in each line is printed with four significant digits and the other numbers in the line with the decimal point in the same location. With more than two independent variables, different pages are devoted to the different levels or combinations of levels of factors three and four. **Availability:** If enough interest develops, a manual will be published by the American Institute of Chemical Engineers, 25 W. 45th Street, New York 36, N. Y. Reprinted from *Chem. Eng. Prog., 1959, 55: 9, 86.*

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Semi-log

$$\log y = A + Bx$$

Quadratic in logarithms

$$\log y = A + Bx + Cx^2$$

Logistic

$$y = KA^{Bx}$$

Gompertz

$$y = K/(1 + \exp(A + Bx))$$

The equation or equations desired in a particular case are indicated on a control card which also contains convergence criteria or iteration limit.

Computer: IBM 704, 8K core, 1 tape (optional-output). **Program language:** Fortran II. **Running time:** This is a function of the number of data points and the amount of extrapolation requested. A typical case of 20 data points and a 10-year extrapolation would run about a minute, including on-line output. **Availability:** A program manual can be made available for publication by the American Institute of Chemical Engineers, 25 West 45th St., New York 36, N. Y., should sufficient interest develop. Reprinted from *Chem. Eng. Prog., 1959, 55: 11, 92.*

Curve Plotting Routine, *H. D. Eddy, Standard Oil Company of California, San Francisco, California* **(CPA 30)**

Description: This program reads a set of X and Y data points (a maximum of 500 pairs), orders them by decreasing values of Y, and prints out a graph of either 50 x 100 or 100 x 100 divisions. Any grid character and plotting character may be used. **Computer:** IBM 704, 4K or larger. **Program language:** Fortran. **Running time:** Time for each problem after the binary deck has been loaded is from six to fifteen seconds, depending upon the number of data points and the size of the output sheet called for. **Comments:** This program is self-sufficient and can read data from cards or from a tape previously prepared from another 704 problem. It may be used to plot the results of the Tarner calculation. **Availability:** This program is presently being used for production, and a complete program write-up, less flow diagram, is available at this time. Flow diagrams can be prepared if this program is accepted for publication by the American Institute of Chemical Engineers, 25 West 45th St., New York 36, N. Y. Reprinted from *Chem. Eng. Prog., 1959, 55: 9, 86.*

Response Surface Evaluation, *Allan W. Dickinson, Monsanto Chemical Company, Applied Mathematics Section, St. Louis, Missouri* **(CPA 31)**

Description: The program evaluates a first or second degree polynomial in two, three or four independent variables, and prints the results in tabular form. Tape output is optional. **Computer:** IBM 704, 4K core, 1 tape (optional). **Program language:** Fortran II. **Comments:** Up to fifteen items may be printed in a single line of the matrix. The largest number in each line is printed with four significant digits and the other numbers in the line with the decimal point in the same location. With more than two independent variables, different pages are devoted to the different levels or combinations of levels of factors three and four. **Availability:** If enough interest develops, a manual will be published by the American Institute of Chemical Engineers, 25 W. 45th Street, New York 36, N. Y. Reprinted from *Chem. Eng. Prog., 1959, 55: 9, 86.*

A Basic IBM 650 Program to Find Best-Fitting Orthogonal Factors for a Given Hypothesis, *D. R. Saunders, Educational Testing Service, Princeton, N. J.* **(CPA 32)**

A program computes a modified quartimax rotation based on a hypothesized factor pattern, which is used to select the subset of variables to which to attend when rotating in a given plane, in order to find an orthogonal solution which fits closely the hypothesis. The program also provides a measure of the goodness of this fit, partial hypotheses as well as hypotheses which specify a determinate or over-determinate simple structure. The program can also be used for a straight Quartimax solution.

The size nk of the factor matrix should not exceed 900, where n is the number of variables and k the number of factors.

A more detailed description of the program and a set of program cards may be obtained from the Educational Testing Service, Nassau Street, Princeton, N. J.

A Basic IBM 650 Program for Paired Comparisons from Balanced Incomplete Blocks, *Harold Gulliksen and Ledyard R. Tucker, Princeton University* **(CPA 33)**

This program processes data obtained by the multiple rank order method devised by Gulliksen and Tucker, in which the subject ranks 3 subsets of 6 stimuli, which are chosen from all 31 stimuli to form a balanced incomplete blocks design. Other designs require small changes in the program. For each subject, the program gives the number of items checked positive and the number negative, the number of votes for each stimulus in comparison with the remaining 30 stimuli, the number of circular triads in the votes for the 31 stimuli, the number of votes for each stimulus and for the implied zero point and the number of circular triads among all preferences.

In addition, the program gives for the total group of subjects the number and proportion of votes for the two stimuli i and j in each paired comparison; the normal deviate, the arc sine and the logistic transform of each proportion; the paired comparison scale values for each of the 31 stimuli based on each of the three transformations; and also the scale values for the 31 stimuli and the implied zero point.

The program will handle up to 999 subjects. Each subject is processed in 35 seconds, the computation for the total group requires an additional 15 minutes.

More detailed descriptions of the method and of the program can be obtained from the authors by writing the Educational Testing Service, Nassau Street, Princeton, New Jersey.

MAPS, a System of Interrelated Programs for Factor Analysis on the IBM 704, *Jonathan D. Wexler*, Arizona State University.* **(CPA 34)**

An interconnected program system was set up for an IBM 704 with 8192-word core storage plus 8192 drum words and 6 tapes to: (1) find product-moment correlations, i.e., correlation coefficients (and also means, standard deviations, and covariances); (2) calculate tetrachoric correlations; (3) evaluate determinants (of moderate size); (4) transpose an original raw data matrix before calculating correlations; (5) obtain principal components by Kendall's method (which corresponds to Thurstone's principal axes factors); (6) rotate factors by Kaiser's Varimax method (programmed by Mr. Donald W. Maurer); and (7) print out the results. The programs can handle fairly large sized matrices, e.g., the tetrachoric and product-moment programs can handle up to 650 variables and 32[illegible] cases. The factor analysis program can process a maximum of 78 variables and the rotation program will work on factor matrices of up to 6000 entries.

This system, called MAPS (Multiple Analysis Program System), was designed so that it would be possible to go in and out of the above programs more or less at will, changing the order and using output of one of them as input for another (within consistency). In particular, this system allows an individual to prepare a set of raw data cards, feed them into the machine, and obtain as final output a set of rotated factors—the machine handles all the intermediate data manipulation so the output from one program is in the correct input format for the next one. It is also possible to start in the "middle" of such a program sequence, e.g., begin with a correlation matrix obtained from some other source, transcribe the matrix onto data cards and then run it through MAPS to obtain principal axes factors (and rotated factors if desired).

The data input to MAPS consists of cards or tape (prepared in suitable formats).

A complete description of these programs is available for $3.00 from the Publications Office, Arizona State University, Tempe, Arizona.

It may be possible for those who do not have access to an IBM 704 to have their problem run at cost on the Arizona State University 704 which is owned by General Electric.

* At present at the University of Chicago.

People, Places, Programs.

A computer program which helps design new digital computers has been developed by *Marc Shiowitz and Associates* of Gardenia, Calif. The program generates complete point-to-point wiring lists from the detailed logical design of the internal operations of the equipment.

A library of IBM 650 programs collected by *Riley Gardner* and *Miss Lolafaye Coyne* of The Menninger Foundation is facilitating data analyses at the Topeka institution. The IBM 650 of the University of Kansas is used which has index registers and floating point hardware. Exploration of relationships between various dimensions of cognitive organization (e.g., leveling-sharpening, equivalence range, scanning, tolerance for unrealistic experiences, field-dependence) has required ever-larger samples of subjects who have participated in up to 11 hours of individual and group testing. Factor analyses have been used as exploratory methods and, more recently, to test specific hypotheses about relationships between principles of cognitive organization. Programs for the principal axes or components solutions have been particularly helpful. The availability of programs for analytical rotation of factors is also an asset, particularly in exploratory studies. The group is looking forward to the development of programs for the newer multivariate techniques and for calculation of factor scores, pattern analysis, and cluster analysis. The latter are particularly relevant to the general problem of "cognitive styles" (patterns of cognitive organization, as contrasted to positions relative to single dimensions of cognitive organization). A recent study, by *Gardner* with *Samuel J. Messick*, of ETS, and *Douglas W. Jackson*, of Penn. State, yielded evidence of relationships between principles of cognitive organization apparent in laboratory and clinical tests and certain "intellectual abilities." Other recent studies have been focussed on relations between single pairs of cognitive principles, e.g., those that have differential effects upon attention deployment in response to certain types of adaptive problems.

On Beethoven's writing-table he kept framed and under glass always before him some sentences copied in his own hand. He had found them in an essay by Schiller having to do with the religion of ancient Egypt. They read as follows: 'I am that which is. I am all, what is, what was, what will be; no mortal man has lifted my veil. He is only and solely of himself, and to this only one all things owe their existence.' That singular desk companion was not there to work magic. He was not one to rely on incantation. It was there as a reminder to himself of where he found the ultimate source of his creative power; that it had spoken to him and through him time and again; and that it would continue to speak.

LUCIEN PRICE, *The Will to Create*

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

SINGER, ROBERT D. **Organization as a unifying concept in schizophrenia.** *A. M. A. Arch. Gen. Psychiat.*, 1960, 2, 61–74.

The data and hypotheses about schizophrenia come from numerous scientific disciplines and although the total research in this area can never fit into any single theory or program, the author introduces a concept by means of which it may be possible to show how the various viewpoints are vitally interrelated.

The leading theories of schizophrenia are presented with interpretive comments. First, the "Genetic Argument" is discussed, particularly the proposition of Kallman who states that schizophrenia may be due to a mutant recessive gene, which might be responsible for a specific type of enzyme deficiency. Further genetic studies cited lead to the conclusion that the relationship between genetic endowment and both structural physiological and behavioral development is highly complex and not some simple function predictable from genetic formulation alone.

The "physiological position" that relates schizophrenia to an underlying organic process is then considered but the meaning of a physiological disorder, if one can be demonstrated, appears to be no clearer than in the case of the genetic argument. The "psychological position" is not representative of theoretical positions per se but includes descriptions of the perceptual, learning, associative, motivational, logical, and linguistic world of the schizophrenic. The "sociological viewpoint" gains its major contribution by demonstrating the differential epidemiology of psychosis and other forms of mental illness. While the data discussed failed to support a strong independent theory, they raise questions about any genetic and physiological position which ignores socio-cultural factors.

The author then presents an integrative viewpoint in terms of the concept of "organization." Organization here refers to transactions between parts of functional systems. The theoretical construct of organization by Rashkis is explained which is a central mediating mechanism dependent on the existence of certain neural mechanisms. Organization is related to psychological, physiological, and the socio-cultural approaches to schizophrenia and is offered as a common way of considering various "levels of analysis." Schizophrenia is defined as an inability to keep the psychological field organized, manifested as an inability to pay attention to task requirements and an inability to coordinate messages from various internal systems so that predictable adaptive behavior may result.

Factors making for organizational strength and therefore a state of mental health incompatible with schizophrenia are considered. Implied is a relationship between organization strength and internal and external stresses as far as the ability to stay non-schizophrenic is concerned. Lack of genetic defect, optimal neural organization, and stable economic conditions would, as indicated by the studies in the relevant areas considered, be helpful. Finally, the psychological conditions leading to optimal organization are suggested. (JCP)

MILLER, JAMES G. **Information input overload and psychopathology.** *Amer. J. Psychiat.*, 1960, 116, 695–704.

Either an excess or a deficiency of stimuli is known to be deleterious to living organisms. Sensory deprivation of normal subjects has produced delusions and other phenomena characteristic of psychotic states. Similarly, the overload conditions under which many

executives operate are believed to contribute to their shortened life span. A systematic research program designed to measure the effects of information overload on various types of living organisms ranging in complexity from a single nerve fibre to whole social institutions is described in this paper.

The experiments are being analyzed within the framework of the general behavior systems theory advocated by the author and some of his colleagues at The University of Michigan. All quantifiable aspects of reaction to information overload are being studied at five different levels of complexity of living systems. These are the cell, organ, individual, group, and social institution. In each case, measurements of performance and mechanisms of adjustment are being made on systems at various input rates to see how these variables are affected by the degree of overload. A comparison of similarities and differences at the different levels will then allow a test of some of the propositions deduced from general systems theory.

The preliminary results presented in this paper confirm some of the earlier deductions. Mechanisms of adjustment at both individual and group levels have been tested on an IOTA apparatus, which flexibly allows the subjects to queue, filter, make omissions and errors, and reduce the number of discrimination categories in an information transmission experiment. The performance at both individual and group levels shows quantifiable similarities. Experiments conducted at the cellular, organ, and social institution levels are expected to reveal some similarities as well as systematic differences, especially in the availability and use of the various mechanisms of adjustment. Research in these areas is still under way. (WJH)

BRADLEY, P. B., DENIKER, P., and RADOUCO-THOMAS, C., Editors. *Neuro-Psychopharmacology*. Amsterdam: Elsevier, 1959, 727 pp., $27.00.

This publication, the Proceedings of the First International Congress of Neuro-Psychopharmacology (held in Rome, September, 1958), is very similar in format to the volume *Psychotropic Drugs* which contains papers presented at the 1957 International Symposium in Milan. The first third of the book contains the papers presented in formal group symposia. The formal sessions included a number of summarizing papers, by some of the outstanding scientists doing research in this field—for example, Bleuler, Elkes, Masserman, Olds—each followed by shorter, formal discussions by eminent scientists such as Delay, Eccles, Freyhan, Himwich, and Kaada. The remainder of the book is devoted to 133 short papers on experimental studies with animals, clinical reports (frequently emphasizing neurological as well as therapeutic findings), and a few papers on experimental changes in normal subjects. Animal experiments and clinical observations on psychotomimetic drugs, including a number of relatively new compounds, such as psilocybin and Sernyl, take up a large part of the book. Fewer than a third of the papers were presented by North Americans. Thus one of the most important features of this book is as an introduction to work that may not be accessible or known in the United States. (LU)

LETTVIN, J. Y., MATURANA, H. R., MCCULLOCH, W. S., and PITTS, W. H. **What the frog's eye tells the frog's brain,** *Proceedings of the Institute of Radio Engineers*, 1959, 47, 11, 1940–1951.

This paper re-examines the hitherto held concept that information is transmitted from the eye to the brain in the form of a mosaic of light which maps images focused on the retina in some sort of geometric pattern in the brain. The authors propose an alternative explanation, that the nervous apparatus of the eye itself is designed to detect certain patterns of light and that the optic fibers transmit only certain operational information about these patterns to the brain. A series of experiments were accordingly conducted on the eye and brain of a frog to determine the nature of this transmitted information.

The conclusions reached were that four separate operations are performed on the image in the frog's eye and that the result

of each of these operations is transmitted by a separate group of fibers, uniformly distributed across the retina. The four operations are (1) sustained contrast detection, (2) net convexity detection, (3) moving edge detection, and (4) net dimming detection. The first two are reported by unmyelinated fibers and the last two by myelinated fibers. The fibers reporting the different operations differ systematically not only in fiber diameter (and therefore conduction velocity) but also in the size of receptive field reported which ranges from about 2 degrees diameter for the first operation to about 15 degrees for the last. The detailed description of these operations as demonstrated experimentally follows.

Sustained Contrast Detectors. This type of fiber does not respond to the light being turned on or off, but if the sharp edge of an object either lighter or darker than the background moves into its field and stops, it discharges promptly and continues discharging no matter what the shape of the edge or whether the object is smaller or larger than the receptive field. The sustained discharge can be interrupted in these axons by switching all lights off. When the light is restored, the sustained discharge begins after a pause. In some fibers of this group, a contrast previously within the field is "remembered" after the light is turned off, since they keep up a low discharge rate that is not present if no contrast was there before.

Net Convexity Detectors. These fibers detect edges with high curvature. The fiber will not respond to a change in general illumination nor will it respond to a straight edge of a dark object moving through its receptive field or brought there and stopped. It does respond to a small object (covering 3 degrees or less at the eye) passed through the field; the response does not outlast the passage. If the object is left in the field, it continues responding for a long time, but the response stops if there is a transient general darkness lasting $\frac{1}{10}$ second or longer. Usually a fiber will respond indefinitely only to objects which have moved into the field and then lie wholly or almost wholly within the receptive field. The discharge is greater the greater the convexity, or positive curvature, of the boundary of the dark object until the object becomes as small as about one half the width of the receptive field (7 degrees in diameter). At this point, the response reaches a maximum and eventually decreases. The smallest object to which these fibers respond is less than 3 minutes of arc.

Moving-Edge Detectors. These fibers respond to any distinguishable edge moving through their receptive field, whether black against white or the other way around. They will respond to an edge only if it is moving. These fibers generally show two or three regularly spaced spikes to turning the light on or off. The response to moving objects is much greater than to changes in total illumination and varies only slightly with general illumination over a range of 1/300. The frequency of the discharge increases with the velocity of the object within certain limits. These fibers are myelinated, conduct at a velocity of 2 meters per second, and have a receptive field about 12 degrees wide.

Net Dimming Detectors. These are myelinated fibers and are the fastest conducting afferents from the retina (10 meters/sec). They respond to a sudden reduction of illumination by a prolonged and regular discharge which is much the same from fiber to fiber. The receptive field is rather large (about 15°), and darkening of a spot produces less response when it is in the periphery of the field than when it is at the center. A response which is set up by turning off a light may be interrupted by turning the light on again. The amount of light which must be restored to interrupt the response gets less and less the longer one waits.

In addition to these four operations, there is a small group of afferent fibers which does not seem to have a distinct receptive field. They measure the absolute degree of darkness over a wide area with a long time constant. The frequency of discharge is greater the darker it is.

There are about 500,000 fibers conveying all of the above information from the retina to the superior colliculus in the brain. The four operational groups of fibers terminate in

four separate layers of cells in the optic tectum. The contrast detectors go to the most superficial layer. Beneath them are the convexity detectors. Then the moving edge detectors mixed with a few of the level detectors. In the deepest layer are the dimming detectors. Thus, the depth at which the fibers end is related directly to their speed of conduction.

The implications of this work are clear. If the optic fibers carry only highly coded information about certain operational characteristics of the visual image to the brain, the derivation of these operational characteristics must take place locally in the retina. For example, the dendritic distribution of the ganglion cells of the different types of fibers may be organized differently so that they obtain different samples of simple local operations reported by the bipolar cells. Further work is under way to explore this aspect of the problem. (WJH)

LURIA, A. R. **Brain disorders and language analysis.** *Language and Speech*, 1958, I, 14–35.

A physiological theory of language function is developed from work with literally thousands of Russian aphasics over the past two decades. Separate regions of the cortex form complicated systems for the analysis and synthesis of visual, auditory, kinesthetic, and motor stimuli. Following Pavlov, Luria calls these "primary cortical analysers." Focal lesions of the brain always produce a break-down in a primary analyser. But this immediate primary effect of a focal lesion never remains isolated. The real units of brain function are complicated systems of activities. When an analyser which participates in any such activity is broken down, normal existence of the whole system becomes impossible, and there is a selective impairment of everything which depends on a given primary analyser. These systemic impairments are the actual symptoms of the aphasias.

Lesions in the left temporal zone cause a break-down in the discrimination and generalization of sound pattern, and above all in phonemic auditory perception. This in turn affects pronunciation and writing, and also the structure of word-meanings. This failure at the lexical level affects the various parts of a word in different degrees: the roots of words, which carry the more concrete meanings, are lost, while suffixes, with their more abstract meanings are retained!

Two classes of processes are distinguished: communication of "relations" and "events." The former are for the most part reversible operations (perception of visual objects, spatial relations, numerical structures); the latter, irreversible serial tasks (reproduction of rhythms, skilled movements, a series of words). It is shown clearly that the former accompany focal parieto-occipital lesions, the latter accompany focal fronto-temporal ones. In the latter, all forms of handling propositions, where the serial order of events is crucial, are impaired.

In all the foregoing disabilities, the pragmatic directive effects of speech on purposeful activity are unimpaired. But in lesions of the frontal cortex, it is precisely these functions which are enormously impaired. The patient cannot use heard or spoken speech to control his movement, he cannot tell himself to do anything, nor respond to request. If asked to procure a farther object, he will fetch a nearer one. Distractability on the simplest tasks is high. Yet he often speaks quite normally, and this seeming normality has often led investigators to neglect the role of language in these pragmatic functions which are a main concern of the Russian school. (WPL)

Life is peculiar, said Jeremy. As compared with what? asked the Spider.

WALPOLE, *The Jeremy Books*

ABOUT THE AUTHORS

Arthur L. Burns ("Theoretical Analysis of the Balance of Power"), a native of Australia, attended the University of Melbourne and is currently a research fellow in international relations at the Australian National University at Canberra. He spent the previous year at the Center for International Studies at Princeton where with his co-authors Kaplan and Burns the present paper was planned and mostly written. His current research interests concern the theory of international relations, military strategy, and power politics in relation to the nuclear age, subjects on which he has published several articles. He has also contributed a chapter, "NATO and nuclear sharing," to the book *NATO and American Security*, edited by Klaus Knorr and published by the Princeton University Press in 1959.

Kenneth Mark Colby ("Experiment on the Effects of an Observer's Presence on the Imago System During Psychoanalytic Free-Association") received his B.S. and M.D. degrees from Yale University, and at present is a training analyst at the San Francisco Psychoanalytic Institute. He is doing research in experimental studies of free-association, primitive dreams, applications of stochastic matrices to change during psychoanalytic treatment, and predictive abilities of analysts. He has written several papers and numerous books, among them *Scientific Methods for the Psychoanalytic Situation*. Dr. Colby writes, "Outside of research and teaching, my interests include family, dogs, golf, gardening, piano, writing, chess, languages, travel, wine, and Markovian chains."

A. Paul Hare ("The Dimensions of Social Interaction") is affiliated as lecturer and research associate with the department of social relations at Harvard University. He also works with the department of psychiatry as associate sociologist for Massachusetts General Hospital. At present, he is involved in research on the prediction and simulation of interpersonal behavior in small groups. Several papers on small group research have been published by Dr. Hare, and a book, *Handbook of Small Group Research*, which he wrote recently, is forthcoming. He writes that "The paper on 'Dimensions' is written to summarize my impression of the basic variables which should be considered in the study of small groups." Dr. Hare did his undergraduate work at Swarthmore and also received a B.S. from Iowa State, an M.A. from the University of Pennsylvania, and his Ph.D. from the University of Chicago.

Carl I. Hovland ("Computer Simulation of Concept Attainment"), Sterling Professor of Psychology at Yale University, is interested in the analysis of learning and concept acquisition processes in humans, with emphasis on simulation of discrimination, classification, and induction phases. A member of the Social Science Research Council committee on Computer Simulation of Cognitive Processes, he helped organize a conference on this topic at RAND in 1958. Dr. Hovland pioneered in the mathematical analysis of information contained in concept formation stimuli and developed simple quantifiable stimuli to use in the experimental study of concept processes. He has "Used machine potentiality as a reference point for evaluating human performance. This led to more and more explicit formulation of the concept learning process and to attempts to simulate it on computers." The author of numerous experimental studies on concept acquisition, and on human learning and retention, he is also the author of books on influence process and mass communication. Dr. Hovland did his undergraduate work at Northwestern and received his Ph.D. from Yale.

Earl B. Hunt ("Computer Simulation of Concept Attainment") is a graduate student at Yale University, where he expects to receive his Ph.D. in 1960. He is interested in research on simulation of human thought processes using digital computers, mathematical and statistical methods in the behavioral sciences, and human learning and problem solving. He is co-author of a previous article with Dr. Hovland on "Preference for Logical Types of Concepts." Mr. Hunt received his B.A. degree from Stanford in 1954.

Morton A. Kaplan ("Theoretical Analysis of the Balance of Power") attended Temple University as an undergraduate, obtained his Ph.D. degree in political science at Columbia University, and is now assistant professor of political science at the University of Chicago. He is co-author of *United States Foreign Policy: 1945–1955* and contributed the chapter "Problems of Coalition and Deterrence" in the book *NATO and American Security*. His present research interests are the theory of international politics, strategy theories, and general systems theory.

Harold J. Leavitt ("Task Ordering and Organizational Development in the Common Target Game"), author of the recent book, *Managerial Psychology*, is professor of industrial administration and psychology at Carnegie Institute of Technology. He completed his A.B. at Harvard, his M.Sc. at Brown University, and received his Ph.D. from MIT. An active student of group relations in industry, he is currently doing research in communication and planning in real and laboratory organizations.

Richard E. Quandt ("Theoretical Analysis of the Balance of Power") is an associate professor of economics at Princeton University and co-author, with Willard Thorp, of the forthcoming book *The New Inflation*. Author of various papers on economics, he is interested in operations research, linear programming, bargaining theory, and statistical and economic theory. After attending Princeton University as an undergraduate in economics, he entered Harvard where he received M.A. and Ph.D. degrees in the same field.

Thornton B. Roby ("Commitment"), a previous contributor to *Behavioral Science* (see the April, 1959 issue), is the author of various articles on learning theory and small group behavior. He is presently conducting research, under an ONR contract, on information processing in small groups and is also studying individual factors in decision making under an NSF grant. An associate professor and research associate at Tufts University, Dr. Roby did his undergraduate work at Trinity College, received an M.A. from Wesleyan University, and his Ph.D. from Yale.

Joseph A. Schlesinger ("The Structure of Competition for Office in the American States") is associate professor of political science at Michigan State University. Concerning his present research, he writes: "I have been studying the careers of major party candidates for the offices of governor and senator. When related to additional work on actual turnover of personnel in public office, the present article on competition provides an analysis of the structure of opportunities for political advancement in the states. In general, my concern with both competition and political careers is with their relation to the dynamics of party organization." Dr. Schlesinger is the author of several articles concerned with politics on the state level. He received his A.M. and Ph.D. degrees in political science from Yale University after completing undergraduate work in economics at the University of Chicago.

Those who won our independence believed that the final end of the State was to make men free to develop their faculties; and that in its government the deliberative forces should prevail over the arbitrary. They valued liberty both as an end and as a means. *They believed liberty to be the secret of happiness and courage to be the secret of liberty.* They believed that freedom to think as you will and to speak as you think are means indispensable to the discovery and spread of political truth; that without free speech and assembly discussion would be futile; that with them, discussion affords ordinarily adequate protection against the dissemination of noxious doctrine; that the greatest menace to freedom is an inert people; that public discussion is a political duty; and that this should be a fundamental principle of the American government.

LOUIS D. BRANDEIS, *Whitney v. California*

The word "system" commonly connotes an orderly arrangement or union of diverse but interrelated parts, all of which move in accordance with some form of controlling principle. The conceptual language through which the controlling principle is anchored and articulated can usually be phrased in terms of the logic of universal laws and generalizations. The present essay takes as its line of departure the logic of universal law including, as it must, definitions of necessary and sufficient conditions. On the basis of these definitions, we can describe the most familiar relation in a system of sociological generalizations, that of correlation or association among variables.

SYSTEM-CONSTRUCTION IN SOCIOLOGY

by Llewellyn Gross

University of Buffalo

THE LOGICAL FORMS OF UNIVERSAL LAW

THE logical forms of universal law have been stated in several different but closely related ways. The following forms of expression are among those most commonly found in analyses of scientific method: "All A is B," "If A then B," "If not B then not A," "If X is an A then X is a B," "For every X, if X has property A then X has property B," "Each a is a b," "If a then b," "If a then B," "If A then b," "Nothing is an A (or a) which is not also a B (or b)." In the analysis that follows, the first of the preceding forms of expression will be adapted. However, in most cases, it can be assumed that the remaining forms of expression are directly applicable.

The logic of sufficient and necessary conditions can be used to explicate the various forms of universal law. A is said to be a sufficient condition for B if, under varying conditions, the presence (occurrence) of A is always found to be accompanied by the presence (occurrence) of B. A is said to be a necessary condition for B if, under varying conditions, the presence (occurrence) of B is always accompanied by the presence (occurrence) of A. Finally, A is both necessary and sufficient for B if, under varying conditions, B is present whenever and only whenever A is present. A and B are then equivalent ($A \equiv B$). The equivalence relation describes that form of the logic of conditions which represents the form of universal law. In every universal law, A is a sufficient condition for B and B is a necessary condition for A. The converse of this is also true (VonWright, 1951, pp. 64–77, 140–151).

Of course, if A occurs in the absence of B and B occurs in the absence of A, then A is neither a sufficient nor a necessary condition for the presence of B. A and B may, nevertheless, be related through partial association. When some occurrences of A are associated with some occurrences of B, or when certain changes in A vary with certain changes in B, causality in the sense of correlation is said to obtain. Causality in this sense cannot usually be expressed in the universal form represented above. The relation between occurrences of A and B describes a tendency only, and the degree of correlation expresses the probability of that tendency. Another way of phrasing this idea is to describe, "A equivalent to B," as a situation in which the same cause is accompanied by the same effect, and to describe, "A correlated with B," as a situation in which the same cause is accompanied by a distributed effect (Von Mises, 1957, pp. 179–181). The latter relation can be expressed as follows: "If A occurs, B occurs in 80% of the cases"; "In 80% of the cases, an instance of A is also an instance of B."

When A stands in a relation of mere sufficiency or correlation with B, all of the

conditions that produce B are not isolated and identified. Some conditions are neglected and these do, on occasion, either produce B or determine some degree of its occurrence. If A is only a sufficient condition for B, then B may occur without A, and if A is merely correlated with B, then some invariable amount of B may occur with some variable amount of A. For these reasons one cannot say that B occurs if and only if A occurs.

When A is both a necessary and sufficient condition for B ($A \equiv B$), and all occurrences of A and B can be given metrical values, and these values related through a constant of proportionality, then a functional equation or law obtains. Thus, to be fully valid, such a law must meet certain requirements. As a first requirement, it should be possible to indicate the respects in which the variables A and B are measurable. Do these variables differ in degrees of intensity, frequency, effectiveness, or some other respects, such that their values can be ordered in series of more or less? When they cannot be so ordered even the crudest kinds of measurement are impossible. Assuming that the variables can be put in serial order, the second requirement arises; that it is possible to construct units for them that are equal, interchangeable, and additive. When the latter requirement is met, fundamental or "extensive" measurement takes place. The way is then open for the establishment of a stable ratio between increases in units of A and increases in units of B. This third requirement, that the proportions of units of A to units of B remain constant under varying conditions, enables us to express the correlational statement as a numerical or quantitative law (Cohen & Nagel, 1934, pp. 289–301; Hempel, 1952(a), pp. 50–79; Cogan et al., 1958, pp. 1–50).

Laws of this type are sometimes said to hold regardless of outside conditions, but this is only an elliptical way of saying that supporting causal conditions (structures) are assumed to remain constant for purposes of systematic analysis. Though unexpressed, these "conditional" constants are the situational determinants of the numerical laws to which we refer. When the magnitudes represented by numerical laws are themselves found to stand in a constant functional relation, such that each can be mathematically derivable from the others, a higher level law or theory obtains. In some cases, variations in the constants of these laws can be further generalized at still higher levels. Higher type laws pertain, of course, to the macroscopic or molar levels of phenomena and not to the microscopic statistical laws of modern physics. Exceptions to the latter occur when the errors of measurement of the magnitudes of two statistical values can be calculated as functions of one another.

A SYSTEMATIC FRAMEWORK FOR SOCIOLOGICAL GENERALIZATIONS

The preceding paragraphs provide a basis for appraising the respects in which it is possible to construct a systematic framework for sociological generalizations. We note, to begin with, that one of the most useful kinds of generalizations found in sociology approximates the correlational form: "If A increases, then B increases"; "The greater the rate of A, the greater the rate of B"; "The more A changes, the more B changes."[1] Now it is evident from our discussion of measurement that if the associations between two or more variables are total, in the sense that any definite increase in one of them enables us to precisely predict the accompanying increases in the others, then such associations are both transitive and symmetrical. In other words, it follows that:

If A increases with B and B increases with C, then A increases with C, and
If C increases with B and B increases with A, then C increases with A.

By virtue of these relations, we could start with any variable in this miniature system and assert a lawful connection with any other variable. Unfortunately, however, the most that can usually be said in sociology is that a given increase in C is associated with some variable increase in A and B. The increase in C may be relatively large

[1] Note the employment of this general form of relation in the theoretical writings of Arnold Rose (1954, pp. 13–24); Hans L. Zetterberg (1954, pp. 16–38); and J. G. March and H. A. Simon (1958, pp. 7–9 and *passim*).

or small depending upon what other variables are at the time influencing it. We may, for instance, know in a general sort of way that an increase in C is associated with an increase in both A and B. However, it is usually very difficult to determine if the combined increase in A and B would add appreciably to the increase in C, over and above the increase in A or B taken singly. Methods similar to the use of multiple and partial correlation within experimental designs can, of course, be introduced, but such designs are best suited to situations of relatively narrow range. They are hardly adequate for building a systematic theory, one that could be used to articulate a wide variety of social phenomena. From a theoretical viewpoint, what is desired are law-like statements representing invariant associations between events of the same type, associations that hold regardless of the changing conditions under which they occur.

Perhaps we can see more clearly the ramifications of this problem if we take a miniature system of four variables as an illustration. The familiar sociological situation with respect to a set of variables A, B, C, D, is such that:

(1) Some increases in A are associated with some increases in B.
(2) Some increases in B are associated with some increases in C.
(3) Some increases in C are associated with some decreases in D.

It is apparent that no inferences can be drawn from these three statements without the addition of one or more premises. We must, to begin with, assume that all of the variables are associated to a high degree. In the absence of contrary knowledge, we specifically assume, by an act of inductive reconstruction, that there is a reasonably "significant" (direct or inverse) relation between the values of each pair of variables in the three statements. Such an act appears to rest on the following premises:

Most increases in variable A are associated with increases in a system-variable S.
Most increases in variable B are associated with increases in a system-variable S.

By assuming that *most* increases in both variables A and B are associated with a system-variable S, we can draw the logical conclusion that:

Some increases in variable A are associated with some increases in variable B.[2]

If the preceding premises are acceptable for statements (1), (2), and (3), they can be used as a basis for drawing the following additional conclusions:

(4) Some increases in A are associated with some increases in C.
(5) Some increases in A are associated with some decreases in D.
(6) Some increases in B are associated with some decreases in D.
(7) Some increases in D are associated with some decreases in A.
(8) Some increases in D are associated with some decreases in B.
(9) Some increases in D are associated with some decreases in C.
(10) Some decreases in D are associated with some increases in A.

Still other inferences may be drawn from this simple system, but the above will be sufficient to illustrate our main point. By grounding our initial list of three statements on explicit premises, we have, through a process of logical derivation, arrived at conclusions (4) through (10). In this way, we have constructed a hypothetical system in which each generalization contributes intelligibility to every other. Can such a framework serve as a basis for systematizing sociological generalizations? It is to the exploration of this question that we now turn.

SYSTEMS OF SOCIOLOGICAL GENERALIZATIONS

In the writings of sociologists, no consistent distinction has been maintained between the two statement forms, "Some increases in A are associated with some increases in B" and "The greater the increases in A the greater the increases in B." We may assume, therefore, that both forms are implied by very similar premises and rest on

[2] As a moment's thought will reveal, the two premises from which this conclusion is derived require acceptance of the more basic premise "Any two variables of which each is associated with more than half of a third variable must be partially associated."

the same kind of relation between variables. The two forms seen to exclude only the possibility that some increase in the one variable will be accompanied by no increase in the other variable. The precise degree of association between variables is left open for future determination. The second form of the statement, like the first, is based on an underlying premise which must be accepted before sociological generalizations can be systematized. This premise may be expressed as follows:

> Any two variables that increase with a third variable increase together.[3]

There are two reasons for utilizing this premise in the organization of sociological generalizations. First, it enables us to simulate the form of law-like statements that are common in natural science, and secondly, it is consistent with the most typical style in which sociological generalizations are presented.

Allowing, then, for occasional abridgments or abbreviations, the following generalizations are representative formulations of the content of sociological writing.[4] Whatever novelty can be found in their organization arises from the juxtaposition of separate groupings of them, each of which intersects or overlaps the region of all others. For instance, if one author relates generalizations (A), (B), and (C) and another author relates generalizations (B), (D), and (E), then a larger scheme of generalizations combining (A) through (E) into a single system is constructible. Further commentary on these generalizations will follow their presentation as listed below.

[3] Expressed synonymously, "Any variable that increases with the first of two variables that increase together increases with the second of these."

[4] Source materials are too extensive to be listed in full. However, the following writings are especially worthy of mention: Broom and Selznick (1958); Cuber (1955); Folsom (1931); Hare, Borgatta, and Bales (1955); Hartley and Hartley (1952); Honigmann (1954); Ogburn and Nimkoff (1958); Phelps (1936); Schuler, Gibson, Fiero, and Brookover (1952); Slotkin (1952); Sorokin (1928); Sutherland, Woodward, and Maxwell (1956); Williams (1947).

Subsystem I

(A) The greater the rate (number) of social (technological) changes
- (1) The greater the increase in size and density of population
- (2) The greater the rate of population mobility and migration
- (3) The greater the rate of social invention and cultural borrowing
- (4) The greater the frequency of social lag and social crises

Subsystem II

(B) The greater the heterogeneity of population
- (1) The greater the number and diversity of sub-groups
- (2) The greater the ethnic and status variability
- (3) The greater the differentiation and specialization of occupations
- (4) The greater the variability of vocational and interest groups
- (5) The greater the diversity in manners, conduct, taste and peculiarities

Subsystem III

(C) The less the uniformity (stability) of social institutions
- (1) The greater the discontinuity in folk traditions and bonds of kinship
- (2) The less the social consensus (shared experience and solidarity)
- (3) The less the agreement on norms (rules, definitions) of mutual obligation (rights and duties)
- (4) The more the centralization of social control (authority)
- (5) The less the informal demand for conformity
- (6) The greater the degree of sub-group autonomy

Subsystem IV

(D) The less the stability (uniformity) of social interaction
- (1) The lower the coordination of role patterns (age, sex, kinship)
- (2) The greater the inconsistency and ambiguity of role patterns
- (3) The greater the fragmentation, seg-

mentation and atomization of social life
(4) The sharper the perception of differences in belief and behavior

Subsystem V

(E) The greater the sub-group conflict (opposition)
(1) The greater the inter-individual stress and strain
(2) The greater the frequency of social deviation (crimes, divorces, suicides, mental disorders)
(3) The greater the antagonism of loyalties

Subsystem VI

(F) The greater the intra-individual conflict (tension)
(1) The greater the conflict of self-conceptions in the individual
(2) The greater the amount of individual anxiety and frustration
(3) The greater the anonymity and isolation of the individual

It is apparent from the way in which the preceding generalizations are grouped that they constitute clusterings of subsystems around the set of principal components designated by the letters (A) through (F). These principal components, dealing as they do with questions of social change, population differences, the stability of social institutions and social interactions, and processes of individual and group conflict, seem to be among the dominant themes of sociology.

Beyond this broad assumption is the additional one that, regardless of their systemic location, any pair or combination of components will constitute a reasonable association.[5] To fully test this assumption, the reader should "ring the changes" on all possible arrangements of generalizations that are constructible from the linking of each component in each subsystem with varying numbers of other components within the total system. Thus, the following subsystem is but one of many that can be constructed in this manner.

(A-2) The greater the rate of population mobility
(B-5) The greater the diversity in conduct
(C-2) The less the social consensus
(D-4) The sharper the perception of differences in belief
(E-3) The greater the antagonism of loyalties
(F) The greater the intra-individual conflict[6]

Not only should each of our generalizations represent a summarization of observational instances, but each, by virtue of its abstract character, should contain that extra measure of conceptual content that permits of extension to relevant classes of unobserved instances. Though a single generalization rests on limited empirical foundations, the systemic linkage of each with the others justifies the interpretation that each will draw upon the combined empirical strength of the system as a whole. Each generalization adds plausibility to the observational instances underlying the others, and the observational instances underlying the others increase the degree of plausibility that can be attributed to each generalization. In this light, any single generalization can be regarded as a parsimonious synthesis of the broad class of empirical phenomena "covered" by the larger system.

Short of the nearly universalized assumption implied by this perspective, the empirically minded researcher may wish to

[5] However, component (C-4) vis-a-vis (C-5), and component (E-1) vis-a-vis (E-2), are sometimes contradictory in the literature. Thus the former may be expressed as, "The less the centralization of social control the more the informal demand for conformity," and the latter as, "The greater the inter-individual stress the less the frequency of social deviation." That the interpretations of empirical relationships are sometimes contradictory in the sociological literature is clearly evident in an article by F. R. Westie (1957).

[6] As our discussion of "methodological problems" will indicate, the indeterminacies of present knowledge are too great to make more than a nominal distinction between principal and secondary components. In the interpretations of H. M. Blalock, Jr. and A. B. Blalock (1959), our principal component, *E*, would probably be regarded as *included in* but not an *element of* the preceding subsystem.

proceed on the more tenable premise that each subsystem constitutes a hypothetical ensemble of generalizations that may apply directly to limited spatial-temporal regions. At least, it may be presumed that many of the sociologists who authored some portion of our list based their generalizations on inferences from direct observations of a local nature.

METHODOLOGICAL PROBLEMS

Assuming a network of systemic relations among generalizations, regions of inconsistency will provide focal points for intensive exploration and analysis. Thus, the structural weaknesses of the larger system can be effectively located and their implications searched as a basis for introducing methodological improvements.

Probably our most visible methodological problem pertains to terminology. If each generalization is meant to refer to a different range of phenomena, then the principal terms of each should be conceptually distinguishable. Though we have, in several instances, bracketed synonymous word usages, some of the terms within the different generalizations carry very similar connotations. Thus, "heterogeneity of population" may be reducible, by equivalent definition, to most of the generalizations subsumed under (B). Similarly, (E) may be no more than a behavioristic summarization of the covert characteristics referred to in (E-3). Again, (C-2) and (C-3) seem to refer to nearly the same kind of data unless one wishes to insist that "social consensus" is implicit whereas "agreement on norms" is explicit. Moreover, do not the meanings of (D-1) and (D-2) provide us with nearly the same interpretation? Perhaps the looseness of language to which we refer is most evident when we consider the possibility of alternative formulations for several of the generalizations. For instance, if we had put in the place of (C-2) and (C-3) the statement, "The fewer the number of common values," would we have omitted any additional types of data relevant to the subsystem in question? Again, if we had substituted either generalization (C) or (E) for the statement, "The lower the degree of social integration," would our formulation have been less definite or less adequate than it now is? Questions of this kind challenge our usual notions concerning the clarity and homogeneity of system variables in sociology.

Without a high degree of clarity and homogeneity among system variables, it is not possible to determine the precise direction(s) in which such variables are causally effective. In a closed system, all variables are mutually determining and one can read the causal relationship as extending from any variable taken as independent to any other variable taken as dependent. The direction of the relationship is formulated on the basis of considerations that have to do primarily with the possibilities of social control. In the absence of criteria of causal effectiveness and system closure, it is difficult to define both the conditions of equilibrium among subsystems and the controlling situational conditions that govern or preserve their boundaries.

The question of equilibrium maintaining conditions within systems cannot be adequately understood without analysis of the problem of how the balance between opposing conditions is maintained. From the literature of sociology we find that subsystem (E) is linked to processes of types (G) and (H).

(E) The greater the sub-group conflict (opposition)

(G) The greater the likelihood of new patterns of avoidance, segregation, stereotyping, discrimination, prejudice, domination, exploitation, elimination, or annihilation

(H) The less the likelihood of new patterns of accommodation, assimilation, amalgamation, coalescence, alliance, cooperation, and co-optation.

Apparently there is some optimum range of variation within the extremes of processes (G) and (H) that prevents the destruction of the larger system of which they are a part. Too much "elimination," for instance, would lead to the destruction of system parts; too much "assimilation" would mean the loss of dynamic adjustment. But this way of stating the problem is too indefinite to provide more than a hint of what is

involved.[7] Generally speaking, equilibrium is most easily achieved when system variables either share a common relation, coordinate the component products of opposing relations, or are united by means of functional relations that complement one another. We have already suggested, by reference to metrical requirements, why the character of such relations cannot be clearly indicated for sociological systems.

Similar obstacles apply to the analytic formulation of the environmental milieu which may be assumed to govern sociological systems. Is our system valid for all societies or only a limited number of them? On what assumption, for instance, do we affirm the universality of social change together with individual diversity and group conflict? Is one of the premises underlying this assumption that of an imbalance between limited resources and services on the one hand and unlimited needs on the other? Is another premise the belief that there are situational constants that serve to forestall the disintegration of systems, that keep society, despite its disequilibrium, from falling apart? Furthermore, what are the goals (end-conditions) that serve to direct both the dominant system and its component subsystems? Are goals multiple and hierarchically ordered, or discrete and loosely structured? In the absence of external influences, is the tendency of every system best represented by the one way process of decreasing asymmetry? (See Whyte, 1949). If this is so, then the asymmetry of the end-conditions must be less than the asymmetry of the initial conditions, except when such end-conditions are directed by subsystems whose symmetries are distorted by the wider system of which they are a part.[8]

Granting that a unitary process applies to the goals of social systems, what are the specific kinds of relations through which it can be described? The goals of social systems are sometimes said to be those of mutual adaptation or adjustment. Is this process describable, then, on the analogy of physical science, as one of cyclical polarization? Frequently, the goals of social systems are taken to be the maintenance or reinforcement of social norms and values, a process often described as dependent upon a hierarchy of dominant-subordinate relations. On other occasions, the goals of social systems are phrased in terms of survival, including the gratification of biological and social needs. For these goals, processes describable through the transaction or transportation of elements from one system to another may be required. Always there is the basic question of whether system relations could be explained by some unknown or partially unknown principle of progressive development like parallel differentiation, self-regulation or self-facilitation.[9] Again, provisional answers to these and related questions will not be forthcoming until more determinate connections can be found between extra-system variables and intra-system variables.

RATIONALE OF SYSTEM-CONSTRUCTIONS

We turn, finally, to the question of how a systems approach to sociological variables may be justified.[10] How, in other words, can fruitfulness be achieved by simulating system-like relations among sociological generalizations when this simulation falls short of scientific adequacy?

In all probability, sociology, along with other disciplines engaged in the task of system building, must seek justification in certain broadly recognized principles of congruence and coherence. The following principles are believed to be among those that underlie every serious attempt to order social phenomena in a systematic way.

(1) System-like generalizations may be held to express certain *ontological principles*

[7] For an informed and seminal analysis of the problematics of interdependence and equilibrium among parts, see the work of Gouldner (1959). Note also an article by D. Easton (1956).

[8] "Asymmetry is treated as the cause of change, and symmetry as the basis of stability. Instability implies asymmetry, and stability symmetry. . . The tendency for asymmetry to decrease is the formative principle necessary to account for the development and stability of regular forms." (Whyte, 1949, p. 23.)

[9] For an illustrated account of the operation of these principles in natural science, see L. L. Whyte (1949, *passim*).

[10] The general relevance of this question is ably discussed by H. Feigl (1950). See also the work by Ashby (1958) and Meadows (1957).

that cannot be shown to be either true or false, on the grounds that there are no events that could count for or against their confirmation.[11] For instance, there appears to be no convincing way in which the statement, "Social changes influence all aspects of culture, society, and personality," can be either verified or refuted. Thus, any social system comprised of culture, society, and personality which incorporates the variable of "social change" appears to articulate a universal metaphysical principle rather than a universal scientific law. Such principles are useful, for though they reaffirm what most professionals know, they are novel to the layman and must be taught anew to each rising generation of students. Supplementing this a priori view is the contention that any statement in sociology can be regarded as either "true" or "false," depending on the sense conferred upon it by the context or system in which it is placed. In this view, tests of the true and the false are construed as relative to specific premises and goals, to the value coordinates that circumscribe the schemes within which they occur.[12] These coordinates consist of general notions of what is relevant, reliable or useful in the light of specific interests, needs, or objectives.

(2) System-like generalizations implementing broad conceptions similar to "the varying heterogeneity of populations," "the instability of social institutions," and "the ubiquity of sub-group conflicts," may be adapted because they are the *organizing principles* upon which all sociological knowledge proceeds. Since they are among the most central notions in the professional language they must enter into every attempt to build an intelligible system of sociological relations. To deny or exclude them would be tantamount to abandoning the field of sociology as an academic discipline. A contrary premise of justification is based on the assumption that every system variable can be joined through a nearly infinite set of relations with other variables. For every variable there are others that either resemble or differ from it in kind, quality, and degree. The purpose of system-construction, then, is to reveal the recurrence or conjugate correspondence of numerous types of related variables, including the specific ways or several senses in which one variable can be taken as the conceptual counterpart, correlate, or complement of others.[13]

(3) System-like generalizations may be joined together in the manner of proposed *rules of reasoning* that are justified on grounds of significance which differ from those of empirical adequacy. Here heuristic, analogical, or nominalistic procedures may play a regulative role. The ends toward which the construction of such rules are directed include the dialectical resolution of intellectual perplexity, the adjudication of claims for analytical or perspectival priority, and the legislation of paradigmatic structures to meet some pragmatic demand. The more extreme view would claim that it is these circumstances and not the search for "objective" scientific truth that induces sociologists to adapt the criteria of appraisal that are normative in their profession. From this viewpoint, most citations of scientific sources are signals of agreement and disagreement, exhortations of what should or should not be done. Note, for instance, how the word "empirical" is constantly introjected into sociological writing without the slightest attempt to

[11] This viewpoint is close to that which R. C. Buck (1956) attributes to the general systems theorists.

[12] No doubt, the professional reader will recognize this perspective as one of many that have been supplied by both American Pragmatism and the range of studies designated as "*Wissenssoziologie*."

[13] Cf. the formulations of Malisoff (1945) and Bertanlanffy (1955). Underlying this general viewpoint is the notion that conceptual systems are free creations of the human mind by which man attempts to cope with his world. Note, for instance, the personal statement of N. Wiener (1959, p. 324): "To me, logic and learning and all mental activity have always been incomprehensible as a complete and closed picture and have been understandable only as a process by which man puts himself *en rapport* with his environment. . . . In this, our main obligation is to establish arbitrary enclaves of order and system. These enclaves will not remain there indefinitely by any momentum of their own after we have once established them. Like the Red Queen, we cannot stay where we are without running as fast as we can."

explore its conceptual foundations.[14] "Empirical truth" may be regarded, therefore, as dependent upon the ceremonially determined correspondences of one verbal context with another.

(4) System-like generalizations may be justified on the basis of *doctrinal judgments* that include private convictions and the habits of academic thought. For instance, if most sociologists believe on faith that variable *A* is correlated with variable *B*, we have a relation between variables which may or may not be determined by the truth possibilities of *A* and *B*. The two variables could both be false, or both true, or either one of them could be false and the other true. These judgments seem to rest on the premise that most co-relations among sociological variables are determined by personal commitments or the probabilities of conventional associations.[15] On this premise, collective usage alone enables us to calculate the "meaning" of a variable at one place in a system from the meaning of other variables at other places in the same system. As a result, the conformity of fact to generalization cannot be proved outside the framework of some conventional set of words. In this sense, connections between variables are not universal, necessary, or immanent. Still, to entertain system-like relations as ideal possibilities, contrived ways of postulating unification among generalizations, does not preclude the likelihood that they may later serve as leading themes for motivating empirical investigation.

(5) System-like generalizations may be justified because there is some small but positive probability that they are *empirically related*. Perhaps the phenomena to which these generalizations refer have been found, on the basis of informal observations, to be more often associated than not. Or it is assumed that even though the empirical association of generalizations has not yet been confirmed, there is analogical evidence that they could provide a minimally adequate account of observed phenomena. For these reasons, they recommend themselves for further investigation. In brief, system relations are justified because they are plausible. As evidence accumulates in support of the proposed relations, they become increasingly likely and finally highly probable.[16] The more extreme view would hold that empirical generalizations are derived from uniformities in perceptual behavior or speech styles, regardless of how these uniformities are ultimately justified. Thus the statement, "All *A*'s are probably *B*'s" is acceptable only when many independent interpreters agree that each one's limited sample exhibits this relation, a sample more accurately described as "Some (all experienced) *A*'s are probably *B*'s." On this interpretation, whatever is repeatable in the communication of men everywhere reflects no more than the uniformities of their human nature.

[14] Though his conceptual orientation differs from the one mentioned here, C. G. Hempel (1952 b; 1959) has had much to say about both heuristic maxims and the limitations of empirical criteria in science. From another quarter, consider the passage from H. Vaihinger (1925, p. 15): "It must be remembered that the object of the world of ideas as a whole is not the portrayal of reality—this would be an utterly impossible task—but rather to provide us with an instrument for finding our way about more easily in this world."

[15] Among the host of writings that are compatible with this viewpoint, the most monumental is probably that of M. Polanyi (1958). The following citation, though typical, belies the huge quantity of "evidence" that Polanyi offers in support of his thesis: "We must now recognize belief once more as the source of all knowledge. Tacit assent and intellectual passions, the sharing of an idiom and of a cultural heritage, affiliation to the like-minded community: such are the impulses which shape our vision of the nature of things on which we rely for our mastery of things. No intelligence, however critical or original, can operate outside such a fiduciary framework."

[16] This terminology comes from C. S. Peirce's famous "Note on the Doctrine of Chances," in which he classifies hypotheses under the headings of plausible, likely, and probable. For a succinct summarization, see Goudge (1950, pp. 199–203).

REFERENCES

Ashby, W. R. General systems theory as a new discipline. *Gen. Systems Yrbk.*, 1958, 3, 1–6.

Bertalanffy, L. V. An essay on the relativity of categories. *Phil. Sci.*, 1955, 22, 243–263.

Blalock, H. M. Jr. & Blalock, A. B. Clarification of system analysis in the social sciences. *Phil. Sci.*, 1959, 26, 84–92.

Broom, L. & Selznick, P. *Sociology*. Evanston, Ill.: Row, Peterson, 1958.

Buck, R. C. On the logic of general behavior systems theory. In H. Feigl and M. Scriven (Eds.), *Minnesota studies in the philosophy of science*. Minneapolis: Univ. of Minn. Press, 1956.

Cogan, E. J. et al. *Modern mathematical methods and models*. Ann Arbor, Mich.: Cushing-Malloy, Inc., 1958, Vol. II, Unit II.

Cohen, M. & Nagel, E. *An introduction to logic and scientific method*. New York: Harcourt Brace, 1934.

Cuber, J. G. *Sociology: A synopsis of principles*. New York: Appleton-Century-Crofts, 1955.

Easton, David. Limits of the equilibrium model in social research. *Behav. Sci.*, 1956, 1, 96–105.

Feigl, H. De principiis non disputandum. . .? On the meaning and the limits of justification. In M. Black (Ed.), *Philosophical analysis*. Ithaca: Cornell Univ. Press, 1950, pp. 125–155.

Folsom, J. K. *Social psychology*. New York: Harper, 1931.

Goudge, T. A. *The thought of C. S. Peirce*. Toronto: Univ. of Toronto Press, 1950.

Gouldner, A. W. Reciprocity and autonomy in functional theory. In L. Gross (Ed.), *Symposium on sociological theory*. Evanston, Ill.: Row, Peterson, 1959, pp. 241–270.

Hare, A. P., Borgatta, E. F., & Bales, R. F. (Eds.), *Small groups*. New York: Alfred Knopf, 1955.

Hartley, E. L. & Hartley, R. E. *Fundamentals of social psychology*. New York: Alfred Knopf, 1952.

Hempel, C. G. *Fundamentals of concept formation in empirical science*. Chicago: Univ. of Chicago Press, 1952. (a)

Hempel, C. G. Problems and changes in the empiricist criterion of meaning. In L. Linsky (Ed.), *Semantics and the philosophy of language*. Urbana: Univ. of Ill. Press, 1952, pp. 163–184. (b)

Hempel, C. G. The logic of functional analysis. In L. Gross (Ed.), *Symposium on sociological theory*. Evanston, Ill.: Row, Peterson, 1959, pp. 271–307.

Honigmann, J. L. *Culture and personality*. New York: Harper, 1954.

Malisoff, W. M. On the possible philosophies of science. *Phil. Sci.*, 1945, 12, 232–235.

March, J. G. & Simon, H. A. *Organizations*. New York: Wiley, 1958.

Meadows, P. Models, systems and science. *Amer. sociol. Rev.*, 1957, 22, 3–9.

Ogburn, W. F. & Nimkoff, M. F. *Sociology*. New York: Houghton, Mifflin, 1958.

Phelps, H. *Principles and laws of sociology*. New York: Wiley, 1936.

Polanyi, M. *Personal knowledge*. London: Routledge and Kegan Paul, 1958.

Rose, Arnold. *Theory and method in the social sciences*. Minneapolis: Univ. of Minn. Press, 1954.

Schuler, E. A., Gibson, D. L., Fiero, M. L., & Brookover, W. B. *Outside readings in sociology*. New York: Thomas Y. Crowell Co., 1952.

Slotkin, J. S. *Personality development*. New York: Harper, 1952.

Sorokin, P. *Contemporary sociological theories*. New York: Harper, 1928.

Sutherland, R. L., Woodward, J. L., & Maxwell, M. A. *Introductory sociology*. Chicago: Lippincott, 1956.

Vaihinger, H. *The philosophy of 'As if.'* New York: Harcourt Brace, 1925.

Von Mises, R. *Probability, statistics and truth*. New York: Macmillan, 1957.

Von Wright, G. H. *A treatise on induction and probability*. London: Routledge and Kegan Paul, 1951.

Westie, F. R. Toward closer relations between theory and research: A procedure and an example. *Amer. sociol. Rev.*, 1957, 22, 149–154.

Whyte, L. L. *The unitary principle in physics and biology*. London: The Cresset Press, 1949.

Wiener, Norbert. *I am a mathematician*. Garden City, N. Y.: Doubleday, 1959.

Williams, R. M. Jr. *The reduction of intergroup tensions: A survey of problems of ethnic, racial and religious group relations*. New York: Social Science Research Council, 1947, Bull. 57.

Zetterberg, H. L. *On theory and verification in sociology*. Stockholm: Almquist & Wiksell, 1954.

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Psychoanalytically oriented social scientists have proposed a "projective" view of the political system in which early family relationships serve as models for adult political behavior. Anthropological field reports indicate that the extension of the family idiom to political relationships is widespread in non-Western cultures, particularly those with stateless political systems. Here the author proposes to show the relevance of social-psychological theories to political anthropology, indicating that cross-cultural testing of the derived hypotheses is feasible using available ethnographic evidence.

THE ROLE OF THE FAMILY IN AUTHORITY SYSTEMS: A CROSS-CULTURAL APPLICATION OF STIMULUS-GENERALIZATION THEORY[1]

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ANTHROPOLOGICAL studies of political organization have suffered both from a lack of testable hypotheses and from too distant a relationship with the other behavioral sciences in which relevant hypotheses have been developed. This theoretical lacuna has resulted in a primary devotion to description and the creation of *ad hoc* taxonomies. Fallers (1956) and Eisenstadt (1956, 1959) have attempted to remedy this situation by the use of sociological theory in their analyses of nonliterate political systems. In this paper I propose to show the relevance of social-psychological theories to the materials of political anthropology and to formulate hypotheses which can be tested cross-culturally.

Psychoanalytically oriented social scientists have suggested that the authority structure of the family is a major determinant of adult political behavior. Parent-child and sibling relationships are seen as models which the individual internalizes and applies to the authority system of the wider society. This theoretical position might be termed a "projective" view of the political system, since it assumes for the political aspect of culture what Kardiner assumes for religion, viz., that the adult beliefs and attitudes are projections of the family situation of early childhood. To what extent is this view valid for any or all societies? Are some political systems more projective than others? What are the social characteristics of more or less projective political systems? These are the questions to which this paper is addressed.

One of the simplest and most fundamental questions we can ask regarding this problem is whether there is respectable ethnographic evidence indicating that this projective phenomenon is at work in any political system. For such evidence we can turn to several British social anthropologists who have studied stateless societies in Africa which are characterized by segmentary lineage systems. A stateless society is typically a human population with considerable linguistic and cultural homogeneity inhabiting a continuous territory, recognizing itself as distinct from surrounding peoples, and lacking a central government. Segmentary lineages are the dominant form of socio-political organization in one type of stateless society.[2] Evans-

[1] An earlier version of this article was presented in a symposium on systems of authority at the American Anthropological Association meeting in Mexico City on December 29, 1959.

[2] A segmentary lineage is typically a patrilineal descent group which is subdivided into segments each of which is itself a patrilineal descent group encompassing several segments and so on, in pyramidal fashion. At the top of the pyramid is the maximal lineage, which may be an exogamous clan or the entire society of several hundred thousands persons; at its base are the minimal lineages, usually extended family units; in between are lineages of varying size and scope. The organization of lineages at all levels is essentially similar:

Pritchard presents the following data on the Nuer perception of their inter-group relationships:

> There are also more general and mythological relationships between clans. In recounting these, Nuer personify the tribes and give them a kinship value. . . Thus, they speak of Bor, Lang, Lou,. . . etc. as though they were persons and could have kinship relations between them like those between persons. . . (1940, p. 237)

Fortes makes the following statement for the Tallensi:

> So all major segments of a lineage of any span are. . . described as "brothers" to one another. Their lineage ties, summed up in the concept "brotherhood," are thought of as being founded on, or derived from, the ties that hold between brothers. (1949, p. 32)

Southall is most explicit about the relationship in his analysis of the Luo lineage system. He says,

> The importance of the family to the lineage is shown by the fact that Luo regard every segmentary relationship as having its symbolic prototype within the compound polygynous family. (1952, p. 6)

He summarizes as follows:

> Throughout, the behavior of segments is largely a repetition on the group plane of certain attitudes of individuals within the family, the people themselves regarding the lineage system as a magnified reflection of the family process. (1952, p. 39)

Finally, Fortes, in reviewing the literature on unilineal descent groups, makes some relevant generalizations.

> We now have plenty of evidence to show how the tensions that seem normally to arise between spouses, between successive generations and between siblings find expression in custom and belief. In a homogeneous society they are apt to be generalized over wide areas of the social structure. (1953, p. 35)

This latter proposition concerning the conditions under which family relationships are generalized, or extended, will be considered in detail later in this paper. At this point suffice it to say that students of segmentary lineage systems have reported that the actors in such political systems conceptualize inter-group relationships in an idiom derived from family relationships.

It might be thought that this extension of the family idiom were limited to segmentary lineage societies, where there is a unique congruence between genealogical and political relationships. In fact, however, such extensions may be found in societies which do not have lineage systems. In those East African societies in which age groups not based on descent are the major social units, relations between age-sets are usually framed in the idiom of father-son or grandfather-grandson relationships. Nor is this phenomenon limited to Africa. The Iroquois conceptualization of their tribal confederation as "the long house," referring to the building which houses their nuclear matrilineage, has long been well known. Oliver reports that among the Siuai people of Bougainville, "the leader is always called a *mumi*, while all other members of the men's society are sometimes referred to as the leader's *kitoria* (children)..." (1955, p. 404). In the prevailing Confucian doctrine of traditional Chinese society, the relationship between emperor and subjects was explicitly modelled on father-son and elder brother-younger brother relations. Examples from societies in different parts of the world could be multiplied. It is clear that the extension of the family idiom to political relationships is a recurrent, though by no means universal, phenomenon.

I am making the assumption, which can be empirically tested, that this linguistic and cognitive generalization reported by ethnographers is assocated with the generalization of attitudes and behavior patterns from the family to the political system. A similar assumption was made by Murdock in his study of kinship, in which he cited psycho-

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a group of men tracing their patrilineal ancestry to a founder. At different levels the lineages differ in size and genealogical depth; they may or may not be territorial units. Segments of a lineage combine for ancestral rituals and military operations against other lineages of the same level, but the same segments may at other times fight one another, at least in some societies. Each lineage, no matter how large, presumably began as the family of its founding ancestor, thenceforth grew and subdivided into segments, and continues to do so. For more adequate and extended definitions, see Evans-Pritchard (1940) and Middleton and Tait (1958).

logical findings to the effect that, "...with human subjects, responses are more likely to be transferred from one stimulus to another when they are given the same name than when they are given different names," (1949, p. 313).

In this paper I shall propose a theory to specify the conditions under which the extension of behavior patterns from the family authority system to the wider political authority system takes place and the conditions under which it does not take place. A contrast between the political socialization process among the Gusii[3] of Kenya and contemporary Americans will serve to introduce this theory.

Two political socialization processes

The Gusii had a stateless political system with segmentary patrilineages and the blood feud. There were no hereditary leadership positions. At the local community level elders jointly made the important judicial and military decisions, with wealthy elders playing a more important role in decision making than those who were poor. A high degree of lineage localization meant that community elders were classificatory or real kin to other members of the community. For the male child growing up in this system, the political socialization process had important continuities. Since patrilocal residence and the patriarchal extended family prevailed, a young man never left the homestead of his father during the latter's lifetime. Thus his relationships with his parents and brothers were not left behind him as he grew up and married but continued to be an important part of his environment. The important figures in the community political system were his classificatory fathers, brothers, grandfathers, and sons. The role that he had learned to play in the authority system of the polygynous extended family was an adequate if not perfect guide to behavior in the community and beyond, wherever lineage ties were operative. Relationships of equal status were assimilated to that of brothers with its informality and competitiveness. Relationships of unequal status were assimilated to that of father and son, with respect and obedience prevailing. Primary socialization, to use Parsons' term, was not entirely distinct from secondary socialization into the political role system, since primary socialization in the family did not have to be supplemented or counteracted by specific preparation for political participation. It is notable and relevant here that the Gusii use the same word, *egesaku*, to refer to the nuclear lineage of a single family head, and to a lineage of any order, an entire tribe, or a nation such as Kenya or the United Kingdom.

By way of contrast we may look at political socialization in the United States or any other complex nation-state. Here, many obstacles stand in the way of an individual's using his role in the family authority system as a guide to behavior in the political system. For one thing, there exists a state with its own rules and expectations of behavior which are different from those of the family. Thus the lessons which the child learns by imitation in the family are unlikely to be applicable to political role-playing. Secondly, American society has neolocal residence and elaborate institutional arrangements for secondary socialization—schools, peer-groups, specialized training programs—all of which provide non-familial role models for the learning of authority relationships. Eisenstadt (1956) has emphasized the role of the age group as a kind of decompression chamber for the individual emerging from his family into the wider social system. In psychological terms we might say that secondary socialization has a retroactively inhibiting effect on the habits learned in the family authority system; it tends to supplant them with new habits more appropriate to political role-playing. Thirdly, the cultural heterogeneity of American society diminishes the impact of primary socialization on the political system. Class, ethnic, and regional groups form subcultural units with differing socialization practices and family authority patterns. Since many aspects of the political system cut across these subcultural units, it is not possible for an individual political participant to assume that the persons with

[3] Field work among the Gusii was carried out in 1955–57 with the support of a Ford Foundation Fellowship and the assistance of my wife, Barbara B. LeVine, who also assisted in the preparation of this article.

h m he is interacting have the same sort of family background as himself. For all of these reasons, then, we are less likely to find Americans extending their family behavior patterns to the political system then we are to find the Gusii doing so.

Transference conditions

Implicit in this comparison are a number of hypotheses about the conditions which facilitate a transfer of habits from the family role system to the political authority system. Several of these hypotheses can be subsumed by the proposition that in stateless societies more of these facilitating conditions are present than in societies with centralized states. For example, there is the hypothesis, consistent with the above statement by Fortes, that cultural homogeneity leads to generalization of family authority pattern, while cultural heterogeneity hinders such generalization. This can be translated into a statement about attributes of the political system when we take into account the correlation between social stratification, which contributes to subcultural variation, and political development. Using Murdock's (1957) classification of a world sample of 518 societies, it is possible to divide them, as in Table 1, into two groups: one with political integration at the local community level or lower, which contains most of the societies usually thought of as "stateless," and one group with supra-community integration, which contains societies with centralized political organizations (states). These can be dichotomized again according to whether or not they possess a class structure (aside from slavery). Those societies with either an hereditary aristocracy or more complex stratification are classified as having a class structure, while those which only have age-grades, individual wealth distinctions, or no stratification are classified as lacking a class structure. Table 1 shows a highly significant degree of association between supra-community political integration and the presence of a class structure, not only for the world sample as a whole but also for five of the six areas of the world on which the hypothesis was independently tested. North America is the only exception. This means that by and large we can expect stateless societies to have less of the subcultural variation to which a class structure gives rise. With less subcultural variation, it should be possible for the actors in stateless political systems, who are homogeneous with respect to family background, to use family roles as a guide to political behavior. This would explain why even those stateless societies which do not have lineage systems are able to use the family idiom in their political concepts, while many state societies are not.

TABLE 1

RELATION BETWEEN CLASS STRUCTURE AND POLITICAL INTEGRATION IN 518 SOCIETIES

(Data based on Murdock, 1957)

Area	Class Structure	"States"	"Stateless Societies"	N	χ^2	P
World (total)	Present	170	32	518	180.5	<.001
	Absent	75	241			
Africa	Present	41	3	111	46.9	<.001
	Absent	18	49			
Circum-Mediterranean	Present	47	2	68	4.9	<.05
	Absent	15	4			
South America	Present	12	4	71	32.6	<.001
	Absent	4	51			
Insular Pacific	Present	21	11	93	22.9	<.001
	Absent	10	51			
East Eurasia	Present	40	1	70	44.6	<.001
	Absent	6	23			
North America	Present	9	11	105	2.8	Not significant
	Absent	22	63			

Other conditions which facilitate or hinder transfer of habits from the family role system to the political role system are the properties of the political environment as perceived by individuals within it. More specifically, the extent of such transfer should vary with the degree of perceived similarity between family roles and political roles. In the language of the psychology of learning, stimulus equivalence must be established for generalization to take place. The more a new stimulus is perceived as

being equivalent to a previously conditioned stimulus, the greater the similarity of the response elicited by the new stimulus to the previously conditioned response. Since Hull (1950) regarded stimulus generalization as a primary social science law, we feel justified in applying it to the problem at hand. The greater the number of stimulus elements which the political environment and family environment have in common, the greater the degree to which individuals will extend their family response patterns to the political sphere of action. The elements which promote or inhibit perception of stimulus equivalence between family and political system can be called, after Murdock's (1949, p. 136) study of stimulus generalization in kinship, social equalizers and social differentials. In this case a social equalizer is an aspect of social structure which creates perceptible similarities between the family and political system; a social differential is an aspect of social structure which increases the perceptible dissimilarities between the family and the political system.

A list of possible social equalizers in this sense would include the following: the presence of corporate descent groups above the family as major social institutions, the localization of descent groups, and the use of kinship terms within territorial units. All of these should, theoretically, act to establish for the individual an equivalence between the family in which he was raised and the wider society, including its political system. Social differentials would include the presence of non-descent groups such as age-groups and secret societies as major social institutions, and the presence of specialized and stable political structures at the supra-community level. The presence of non-descent groups should serve to make the wider society distinct from the family, and specialized, stable political structures should make politics a clearly differentiated form of behavior with norms of its own.

On the basis of the above theoretical framework, it is possible to formulate some testable hypotheses, using as the dependent variable the *congruence of community and supra-community authority patterns with family authority patterns*. Four such hypotheses follow.

1. The degree of congruence is inversely related to the amount of social stratification.

2. The degree of congruence is inversely related to the specialization and stability of supra-community political structures.

3. Among societies of a given level of political integration (such as the local community or nation level), the degree of congruence is directly related to the presence of corporate descent groups, the degree of localization of descent groups, and the use of kinship terms within territorial units.

4. Among societies of a given level of political integration, the degree of congruence is inversely related to the presence of procedures for secondary socialization such as schools, institutionalized peer groups, and military training programs, through which actors in the political system must pass.

I believe that it is possible to devise means of testing these hypotheses using available ethnographic evidence. My purpose in this paper has been to demonstrate that social-psychological theory, when applied to the materials of political anthropology, can yield propositions which are worthy of cross-cultural investigation and which may advance our understanding of political behavior.

REFERENCES

Eisenstadt, S. N. *From generation to generation: Age groups and social structure*. Glencoe: The Free Press, 1956.

Eisenstadt, S. N. Primitive political systems: A preliminary comparative analysis. *Amer. Anthropologist*, 1959, 61, 200–220.

Evans-Pritchard, E. E. *The Nuer*. London: Oxford University Press, 1940.

Fallers, L. A. *Bantu bureaucracy*. Cambridge, United Kingdom: W. Heffer & Sons, 1956.

Fortes, Meyer. *The dynamics of clanship among the Tallensi*. London: Oxford University Press, 1949.

Fortes, Meyer. The structure of unilineal descent groups. *Amer. Anthropologist*, 1953, 55, 17–41.

Hull, Clark L. A primary social science law. *Scientific Monthly*, 1950, 71, 221–228.

Middleton, John & Tait, David (Eds.). *Tribes without rulers: Studies of African segmentary systems*. London: Kegan Paul, 1958.

Murdock, George P. *Social structure*. New York: Macmillan, 1949.

Murdock, George P. World ethnographic sample. *Amer. Anthropologist*, 1957, 59, 664–687.

Oliver, Douglas. *A Solomon Island Society*. Cambridge: Harvard University Press, 1955.

Southall, A. W. Lineage formation among the Luo. *Int. African Inst. Memorandum XXVI*, 1952.

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Master Pangloss. . . could prove to admiration that there is no effect without a cause, and in this best of all possible worlds the baron's castle was the most magnificent of all castles, and my lady the best of all possible baronesses.

"It is demonstrable," said he, "that things cannot be otherwise than they are; for as all things have been created for some end, they must necessarily be created for the best end. Observe, for instance, the nose is formed for spectacles; therefore we wear spectacles. The legs are visibly designed for stockings; accordingly we wear stockings. Stones were made to be hewn and to construct castles; therefore my lord has a magnificent castle; for the greatest baron in the province ought to be the best lodged. Swine were intended to be eaten; therefore we eat pork all the year round. And they who assert that everything is *right*, do not express themselves correctly; they should say that everything is *best*."

VOLTAIRE, *Candide or the Optimist*

What is important to emotionally disturbed patients? Does a schizophrenic with paranoid trends conceive of the good life in the same way that a normal person does? Here values of a group of patients are compared with those dominant in the culture in which they live. Comparisons are also made of values among persons with different types of disturbances—a quantitative study which parallels clinical observations of these patients. Investigating value patterns in emotionally disturbed persons may provide new insights into the characters of normal and above-normal persons.

VALUES OF PSYCHIATRIC PATIENTS

by Charles Morris, Bernice T. Eiduson, and Denis O'Donovan[1]

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INTRODUCTION

THE interest of this study is in comparing the values of a group of psychiatric patients with the values dominant in the culture in which they live, and in comparing the values found in persons with different types of disturbance. *Varieties of Human Value* (Morris, 1956) studied the "conceptions of the good life" of college students in a number of cultures. The basic material used for this purpose was the preference ratings of these students on 13 possible "ways to live." The students indicated their degree of liking of the alternatives by assigning to each a numerical expression from 7 to 1, ranging from extreme liking, through indifference, to extreme disliking. Since it was shown that these numerical expressions could be treated as integers, statistical comparisons could be made across cultures and between various groups within a culture. The present study extends these methods into the field of the mentally ill. The work is conceived as distinctly exploratory in nature. Its purpose is to inquire whether the study of values and value patterns can suggest meaningful hypotheses in the study of personality disturbances.

The main subjects were 50 outpatients of the clinic of the Hacker Foundation for Psychiatric Research and Education. Each of these rated the 13 "ways to live."[2] Members of the staff of the clinic diagnosed the patients and assigned each of them a rating as to the degree of severity of their disturbance. A brief description of their symptoms was also furnished. In addition to this material, the spouse of each patient rated the 13 "ways to live," or, if the patient was not married, a rating was secured from the

[1] The aid of the following persons is gratefully acknowledged. From the Hacker Clinic Associates (Beverly Hills, California): Frederick J. Hacker, M.D.; Elizabeth A. Beale, Ph.D.; Weldon C. Bembower, M.D.; John R. Black, M.D.; Esther Gordon Cohen, M.S.S.; Marcel Frym, J.D.; Eleanor R. Gunter, M.A.; Werner Koenig, M.D.; M. John Schumacher, M.D.; Richard E. Timmer, Ph.D.; and Helen G. Tomkins, M.S.S. From the Department of Psychology, University of Florida: Professors Richard J. Anderson, Bradford N. Bunnell, Malcolm H. Robertson, and Audrey Schumacher. From the Statistical Laboratory of the University of Florida: Professor Herbert A. Meyer and his associates, especially Professor Ernest J. Lytle. Dr. Rahe B. Corlis, Mr. William H. Wharton, and Mr. Donald M. Hartsough also contributed their services. The collection of data was in part sponsored by the Hacker Foundation for Psychiatric Research and Education, Beverly Hills, California.

[2] In the main, the Ways were given as part of the battery of psychological tests (The Wechsler-Bellevue Intelligence Test, The Rorschach Test, The Thematic Apperception Test) when the patient was being diagnostically evaluated in order to determine the nature of his emotional disorder and the need for psychotherapy. About one-quarter of the subjects were already in psychotherapy when the Ways document was presented to them. All subjects were instructed to fill out the document at home, and return it to the clinic. They were also instructed to have their spouse or closest friend fill out the document.

"closest friend" of the patient. Finally, a control group of 50 persons was formed, matching the 50 patients with respect to sex and age. Since the patients lived in a large urban area, the control group was limited to persons living in such areas. Thus the total group of subjects was 150, equally divided into patients, spouses or friends, and controls.

Twenty-three of the patients were men. They ranged in age from 27 to 44, with a mean of 32.7, a median of 33, and a mode of 29. Male controls matched male patients in age precisely.

Twenty-seven of the patients were women. They ranged in age from 23 to 45, with a mean of 34, a median and a mode of 33. Female controls were matched person-for-person with differences in age of less than two years, preserving the same mean and median.

Ages of spouses and friends fell within the same range, with the exception of one husband of 57 and the only parents included in the study, two mothers of 50 and 58 and a stepmother of 53. The median age was again 33.

The "Ways to Live" document which was rated by these 150 persons is given in full in *Varieties of Human Value* (Morris, 1956). Table 1 gives a brief description of the central emphasis of each of the Ways. These descriptions were not given to the subjects, but will indicate the range of values here under consideration.

TABLE 1
ESSENTIAL EMPHASES OF THE WAYS TO LIVE

Way 1: preserve the best that man has attained
Way 2: cultivate independence of persons and things
Way 3: show sympathetic concern for others
Way 4: experience festivity and solitude in alternation
Way 5: act and enjoy life through group participation
Way 6: constantly master changing conditions
Way 7: integrate action, enjoyment, and contemplation
Way 8: live with wholesome, carefree enjoyment
Way 9: wait in quiet receptivity
Way 10: control the self stoically
Way 11: meditate on the inner life
Way 12: chance adventuresome deeds
Way 13: obey the cosmic purposes

TABLE 2
MEAN RATINGS OF THE WAYS FOR PATIENTS, CONTROLS, SPOUSES AND FRIENDS

Ways	Patients (50)	Controls (50)	Spouses and Friends (50)
1	4.80	5.56	4.74
2	2.82	2.68	2.96
3	3.94	4.70	3.84
4	3.72	3.66	3.88
5	4.00	4.02	3.94
6	3.84	4.56	4.22
7	5.62	5.90	5.54
8	4.06	4.36	4.48
9	2.80	3.04	2.48
10	3.06	3.62	3.38
11	2.50	2.76	2.78
12	3.78	4.00	3.64
13	2.66	2.43	2.76
Sum	47.60	51.29	48.64

THE THREE GROUPS CONSIDERED AS WHOLES

The first striking fact to emerge in this study is the general similarity of the value profiles of the patients, the spouses and friends, and the controls. The mean ratings of these groups for the 13 Ways are given in Table 2.

The Pearsonian correlations between the means of the various groups are high: .96 between patients and controls, .94 between spouses or friends and controls, .97 between patients and spouses or friends. Thus the general value profiles show little difference. A further measure of this difference is given by the D statistic:[3] the D's corresponding to the above r's are 1.55, 2.01, and .84. The D of 1.55 between patients and controls is less than the D's between cultures (see Morris, 1956, table 9, p. 43), and comparable D's are found between various groups within the United States itself (Morris, 1956, pp. 72, 84, 104, note 10). Since the control group has a correlation of .97 with a large sample of college students (N = 2,846),[4] the patients, and their spouses or friends, clearly fall within the dominant American value

[3] The D is found by taking the differences between means, squaring the differences, adding the results, and taking the square root of the resulting sum. (See Osgood and Suci, 1952.)

[4] See last column of Table 22, p. 72, in Morris (1956).

pattern. They do not, in general, repudiate the values of their culture; they are for the most part simply having difficulty in achieving them.

Nevertheless, certain differences between the patients and the controls are important. The patients on the whole like the Ways less than do the controls: the average rating in the first case is 3.63, and in the second case 3.94. The distribution of ratings by patients, regardless of the Way being rated, is weighted toward the dislike end, and this difference is significant at the .05 level (Kolmogorov-Smirnov test).

Secondly, the patient group is considerably less homogenous in their likings than the control group. Only five ways received more than two ratings of 7 ("like very much") in the control group, while ten Ways received more than two ratings of 7 in the patients. And in general the standard deviations of the ratings of the Ways were larger in the patients than in the controls. Total liking, i.e., the total number of points given to all thirteen of the Ways by a given person, varied much more with patients than with controls, and tended to agree closely with total likings of their spouses and friends.

The most important difference between patients and controls is on Factor *A*. The score on Factor *A* is the sum of the ratings of Ways 1, 3, 6, and 10 minus the rating of Way 4. Factor *A* may be characterized as "social restraint and self-control." A fuller description is as follows: "The stress is upon responsible, conscientious, intelligent participation in human affairs. The orientation is primarily moral. There is awareness of the larger human and cosmic setting in which the individual lives and an acceptance of the restraints which responsibility to the larger whole requires. The accent is upon the appreciation and conservation of what man has attained rather than upon the initiation of change. The antithesis of the trait is unrestrained and socially irresponsible enjoyment" (Morris, 1956, p. 33).

The patients as a whole are markedly lower on Factor *A* than are the controls. The chi-square for the difference between the scores on Factor *A* of all patients and all controls is significant at the .01 level. The difference is largest in the case of the males: the average score on Factor *A* of the male controls on a 7-point scale is 5.2 while that of the male patients is 3.6.

In summary, the patients as compared with the controls show less liking for the Ways as a whole, are less homogeneous as a group in their values, and value less the Ways which stress behavior under the control of social norms and the needs of others. Hence while the order in which the 13 Ways are liked by the patients is much like the order of liking in the controls and in Americans generally, there are nevertheless important differences in the values of the two groups. In particular the values of the patients are more egocentric and less socially oriented and socially controlled.

This latter finding may be a first hint that the Ways are picking up certain differences in personality makeup despite the patient group's general adherence to the larger cultural value pattern. Their lower score on Factor *A* may be reflecting the temporary turning to the self that comes whenever a patient is beginning to examine his own behavior and personality patterns that have been urged upon him by his symptomatic problems. On the other hand, the lower score on Factor *A* may be a manifestation of difficulties that have been apparent in other spheres of life.

TABLE 3

MEAN RATINGS OF WAYS BY MAJOR GROUPS

(*N*)	Group	1	2	3	4	5	6	7	8	9	10	11	12	13	Sum
18	Neurotics	5.28	2.61	4.00	3.78	4.17	3.78	6.22	4.94	3.11	3.11	2.56	4.06	2.78	50.40
17	Personality disorders	4.41	2.94	4.35	3.59	3.94	3.47	5.29	3.82	2.82	3.29	2.24	3.35	2.41	45.94
12	Schizophrenics	4.67	2.83	3.25	3.58	3.83	3.83	4.92	3.50	2.58	2.92	2.58	3.83	2.83	45.17

SUB-GROUPS AMONG THE PATIENTS

While the score on Factor *A* distinguished quite well the patients as a whole from the controls (and that is its merit), it does not significantly distinguish between sub-groups of patients. Table 3 gives the mean ratings of the Ways for three patient sub-groups: personality disorders, neurotics, and schizophrenics.[5] Table 4 gives the rank order correlations between these three sub-groupings.

It is evident from these tables that the over-all value patterns of these sub-groups have much in common. But again there are interesting differences between them.

The *E* between the neurotics and the schizophrenics is 2.24, which is larger than the *D* between the patients as a whole and the controls. This difference, however, is largely determined by the higher liking by the neurotics of two of the Ways—Way 7 (integrate action, enjoyment, and contemplation) and Way 8 (live with wholesome, carefree enjoyment). The differences on these two Ways are statistically significant. The score for Factor *A* is higher for the neurotics than the schizophrenics, and so is the total liking for the 13 Ways. The Ways which involve interaction with other persons are all higher for the neurotics (Way 3, show sympathetic concern for others; Way 5, act and enjoy life through group participation; Way 8, live with wholesome, carefree enjoyment). While the patients as a whole are more egocentric than the controls, the schizophrenics are more egocentric than the neurotics (See Table 8).

Nothing clear stands out in the group of personality disorders. Their ratings do not vary systematically with respect to the neurotics or schizophrenics.

Table 5 gives the mean ratings of the 13 Ways for a further breaking of the patients into 7 sub-groups. Because of the small size of these groups, few statistically significant results can be claimed. Some trends, however, can be noted as hypotheses for further work.

[5] All the psychotics in the sample were schizophrenics. Hence the use of this term here. The *N* of 47 in Tables 3 and 4 is due to the exclusion of the 3 psychophysiological patients, who were not assigned to one of the three groups here considered.

TABLE 4

RANK ORDER CORRELATIONS BASED ON MEAN RATINGS OF 13 WAYS BY DIFFERENT GROUPS

	Control	Schizophrenics	Personality Disorders
Neurotics (18)	.89	.86	.90
Personality disorders (17)	.92	.85	X
Schizophrenics (12)	.83	X	X

The hysterics

The term 'hysteric' is not used in the American Psychiatric Association classification. We have retained that classification in this study, however, because the number of subjects was not sufficient to break the category down into the newly proposed groupings. We have considered five patients with psychophysiological involvements separately, although in the old diagnostic schema these would have been considered as hysterics. The similarity of the two groups is apparent in Table 5, where the sum of mean ratings is uniquely high. All of those here called hysterical are labeled X01 or X04 in the A.P.A. classification.[6] They are patients 1, 10, 11, 17, 24, 25, 34, 45.[7] None of these patients had any physiological involvements as primary symptoms.

The hysterics give a higher total rating to the 13 Ways than any other group (though only slightly higher than the physiological group). The high mean rating of Way 8 (live with wholesome, carefree enjoyment) is noticeable, distinguishing them most sharply in this respect from the more dependent passive aggressives. In terms of rank order, the hysterics rate Way 2 (cultivate independence of persons and things) lower than any other group (though Way 2 is generally ranked low by all the groups). They in general think highly of Ways which relate

[6] "X01", etc., are abbreviations for the complete classifications given in the *Diagnostic and Statistical Manual* of the American Psychiatric Association.

[7] Copies of the original data on these patients and the other patients can be obtained from the authors.

TABLE 5

MEAN RATINGS OF WAYS BY SOME SELECTED GROUPS OF PATIENTS

(*N*)	Group	1	2	3	4	5	6	7	8	9	10	11	12	13	Sum
8	Hysterics	5.25	2.38	4.88	3.88	4.88	4.88	6.00	5.62	3.00	3.88	2.38	4.63	3.50	55.13
8	Obsessive compulsives	5.25	3.13	3.25	3.00	3.50	2.50	6.25	4.00	3.38	2.50	2.75	3.75	2.25	45.50
5	More aggressive passive aggressives	4.40	3.40	4.00	4.20	4.60	4.00	5.00	5.00	2.40	3.00	2.20	3.40	2.20	47.80
5	More dependent passive aggressives	4.60	2.40	5.00	2.60	4.80	2.40	5.40	2.80	3.00	3.40	2.00	3.40	2.40	44.20
6	Schizophrenics with paranoid trends	5.00	3.33	3.50	3.17	4.17	3.50	4.17	3.17	2.67	3.50	2.83	4.33	2.83	46.17
6	Other schizophrenics	4.33	2.33	3.00	4.00	3.50	4.17	5.67	3.83	2.50	2.33	2.33	3.33	2.83	44.17
5	Psychophysiologically involved patients	4.00	2.40	4.60	5.20	5.60	5.40	6.20	4.80	3.40	2.40	1.40	6.00	3.60	55.00

them to other people, and which stress activity and enjoyment.

The obsessive-compulsives

The patients forming this group are X05's or X06's. Their numbers are 3, 4, 6, 12, 18, 31, 41, 43. The obsessive-compulsives give a high place to Way 1 (preserve the best that man has attained) and give in general a higher rating than the other groups to Way 2 (cultivate independence of persons and things), Way 9 (wait in quiet receptivity), and Way 11 (meditate on the inner life), and give their lowest place to Way 13 (obey the cosmic purposes). They value activity and sensuous enjoyment much less than do the hysterics, and they value control and detachment much more. From a clinical point of view these differences bear out the intellectual orientation and the emphasis on independence which distinguishes the obsessive-compulsives from the dependent hysterical personality.

More aggressive passive aggressives

The patients in this group are numbers 2, 28, 37, 39, 49. They are all labeled as X52. The rating of the more aggressive passive aggressives on Way 8 (live with wholesome, carefree enjoyment) is second only to the neurotics. Way 5 (act and enjoy life through group participation) is ranked third. The ratings for Way 9 (wait in quiet receptivity) and Way 13 (obey the cosmic purposes) are the lowest for any group. The score on Factor *A* is considerably less than for the hysterics. So restraint and receptivity are low in value, while action and enjoyment are high.

The dependent passive aggressives

This group is composed of patients 7, 13, 26, 28, and 50. They are all labeled as X52. The highest rating for Way 3 (show sympathetic concern for others) is given by this group, and the lowest ratings for Way 4 (experience festivity and solitude in alternation), Way 6 (constantly master changing conditions), and Way 8 (live with wholesome, carefree enjoyment). They differ from the aggressive passive aggressives on lower ratings of Way 2 (cultivate independence of persons and things), Way 4 (experience festivity and solitude in alternation), Way 6 (constantly master changing conditions) and Way 8 (live with wholesome, carefree enjoyment). They value sympathetic concern, and their orientation is non-egocentric (in the sense that Ways 2 and 4 are egocentric). The *D* between the dependent and the aggressive passive aggressive groups is 3.58.

Schizophrenics with paranoid trends

The patients in this group are 19, 20, 21, 33, 35, and 46. They are all classified as X24 or X26. The paranoid schizophrenics and those chronic schizophrenics with paranoid trends give the highest ranks to Way 1 (preserve the best that man has attained), Way 10 (control the self stoically), and Way 12 (chance adventuresome deeds), and they give (together with the obsessive-compulsives) the highest ranking among the patient groups to Way 2 (cultivate independence of persons and things). This is in line with the ideational emphasis which is recognized among paranoid schizophrenics clinically. In contrast, they rank lower than any patient group Way 7 (integrate action, enjoyment, and contemplation), Way 8 (live in wholesome, carefree enjoyment), and Way 9 (wait in quiet receptivity). This adds up to a picture of persons who value both self-control and the ability to be adventuresome and independent. Receptivity and enjoyment are little valued.

Schizophrenics without paranoid trends

The patients in this group are 14, 16, 23, 29, 42, and 48. They are diagnosed as X25 or X26. These schizophrenics without paranoid trends differ from those with paranoid trends in ranking higher Way 4 (experience festivity and solitude in alternation), and Way 8 (live with wholesome, carefree enjoyment), and in ranking lower Way 2 (cultivate independence of persons and things), Way 10 (control the self stoically), and Way 12 (chance adventuresome deeds). They also give the lowest rank of any of the patient groups to Way 3 (show sympathetic concern for others) and Way 5 (act and enjoy life through group participation). They also rate Way 7 much higher than do the paranoid schizophrenics. The resulting picture is that of persons essentially seeking to enjoy themselves without regard for other persons and without responsibility to social norms. They differ considerably from the more paranoid schizophrenics. The D between the two groups is 2.94, which is larger than the D between neurotics and schizophrenics as a whole (D = 2.24).

Psychophysiologically involved patients

These are patients 1, 6, 8, 15, and 45. These are in order classified as X01, X05, 6-580, 6-580, X01. Composed of two ulcer cases and three patients from previously-considered groups with somatic complaints as a chief form of expression, this group is striking in one respect. They rank highly the Ways which involve activity and physical energy (Way 5, act and enjoy life through group participation; Way 6, constantly master changing conditions; Way 12, chance adventuresome deeds). They give the lowest rank of any of the patient groups to Way 1 (preserve the best that man has attained), and to Way 2 (meditate on the inner life). The value emphasis is markedly toward the expression of physical activity and away from self-restraint, introspection, and social responsibility. This is in line with clinical observation that such persons tend to find expression for their problems through somatization and bodily expression, rather than through ideational representation. The contrast of this group with the non-paranoid schizophrenics is marked. The D between the mean ratings of the two groups is 4.56, as contrasted to the D between the patients as a whole and the controls (D = 1.55). The total sum of the ratings of the Ways is much higher for the psychophysiologic group (and for the hysterics) than in the other patient groups.

MOVEMENTS AGAINST, TOWARD, AND AWAY FROM PEOPLE

Karen Horney (1945) has suggested three basic temperamental traits as movement against people, movement toward people, and movement away from people.[8] She views the neuroses as "ways of life" developed when these basic trends come into conflict; the neuroses are regarded as attempts to solve such conflicts (Horney, 1950).

Table 6 gives the mean ratings of the 13 Ways of three groups of patients classified by six judges according to the principal complaints about which the patient sought help at the Hacker Clinic. The rating of the

[8] Using the Horney classification here does not imply theoretical agreement with her view about the etiology of neurotic disorders.

TABLE 6

MEANS ON WAYS FOR GROUPS WHOSE SYMPTOMS INDICATE MOVEMENT PRINCIPALLY AGAINST, TOWARD, OR AWAY FROM PEOPLE (IN HORNEY'S TERMINOLOGY)

(*N*)	Group	1	2	3	4	5	6	7	8	9	10	11	12	13
17	Against	5.24	3.00	3.18	2.82	3.41	3.41	6.00	3.59	2.71	2.71	2.82	3.29	2.59
14	Toward	5.00	2.57	4.64	4.43	5.00	4.36	5.86	4.57	2.93	3.64	2.86	4.57	2.57
10	Away	3.50	3.10	3.20	4.20	2.90	3.60	4.80	3.20	2.70	3.00	2.10	3.90	2.10

Ways was of course not considered in making the classifications. The *D*'s between these groups (given in Table 7) are considerable, larger in fact than the *D*'s between American college students and students in India, Japan, China, and Norway (Morris, 1956, Table 9, p. 43). The Ways as a whole are liked considerably more by the Towards than by the other groups, and liked least of all by the Aways. The Against group has a disliking (a mean less than 4.00) for every Way except Ways 1 and 7; the same is true for the Away group except for Ways 4 and 7. The Towards have the highest score on Factor *A* (social restraint and self-control) and the Aways the lowest score. In terms of means, the Towards are higher than the other groups on the Ways which stress action (Ways 6 and 12), enjoyment (Ways 4 and 8), and responsibility to other persons and to society (Ways 3 and 10); they are lower than the other groups on Way 2 (cultivate independence of persons and things). The Away group is higher than the Against group on Way 4 (experience festivity and solitude in alternation), and lower on Way 1 (preserve the best that man has attained) and on Way 7 (integrate action, enjoyment, and contemplation). These results show that the Horney classification finds a reflection in the rating of the Ways.

In general, patients classified as moving towards people were hysterics and more dependent passive aggressives; those moving against were obsessive-compulsives, more aggressive passive agressives, and some of them had paranoid trends; those moving away were schizophrenics or schizoid personalities. There was considerable overlap in formal diagnostic category, however.

TABLE 7

D'S FOR AWAYS, TOWARDS, AND AGAINSTS

	Away	Against
Toward	3.79	3.54
Away		2.87

EGOCENTRICITY VERSUS SOCIALITY

It was stated earlier that the patients as a whole are more egocentric than the controls. The egocentricity vs. sociality dimension is derived from an unpublished study of Charles E. Osgood, Charles Morris, and Edward Ware, in which a factor was obtained that was high on Way 3 (show sympathetic concern for others), Way 1 (preserve the best that man has obtained) and Way 8 (live with wholesome, carefree enjoyment), and low on Way 2 (cultivate independence of persons and things), and Way 4 (experience festivity and solitude in alternation).[9] Factor scores on this dimension were obtained for various groups by simply adding the means for Way 1, 3, and 8 and subtracting the means for Ways 2 and 4. Table 8 gives the results.

It may be noted that while the patient group as a whole is more egocentric than the controls, the difference is not large in the case of female patients and neurotics (of which 12 out of 18 are women). In contrast, the male patients are much more egocentric than the controls. The Away, Against, and Toward groups are egocentric in that order, the Away group being in fact the most egocentric of all the groups. The schizophrenics, personality disorders, and neurotics are egocentric in the order given. The

[9] The order of the Ways from egocentric to non-egocentric was 2, 4, 12, 6, 13, 11, 5, 10, 9, 7, 8, 1, 3.

TABLE 8

MEAN SOCIALIZED (VS. EGOCENTRIC) RATINGS OF THE WAYS FOR DIFFERENT GROUPS

(*N*)	Group	$\bar{X}$
50	Patients	6.26
50	Spouses and friends	6.22
29	Spouses	6.00
21	Friends	6.53
50	Control	8.28
23	Control male	8.86
27	Control female	7.77
23	Male patients	4.87
27	Female patients	7.44
17	Against	6.19
14	Towards	7.21
10	Away	2.60
18	Neurotics	7.82
17	Personality disorders	6.05
12	Schizophrenics	5.01
3	Psychophysiological disorders	3.00

spouses and friends are about the same as the patients in egocentricity; the friends being slightly less egocentric than the spouses.

DEGREE OF SEVERITY OF DISTURBANCE

It has been mentioned that a degree of severity rating was given to each person by the Hacker Clinic. They are given in the expressions 1b, 1c, 2a, 2b, 2c, 3a, 3b.[10] In general these represent an increasing order of severity. The numerals indicate the degree of severity of disturbance in the intrapersonal dynamics and the small letters the degree of severity of disturbance in interpersonal relations. The present data included no cases of 1a or 3c.

It will also be recalled that a *D* was obtained between each patient's rating of the Ways and the ratings of the patient's spouse (or close friend). It seemed a plausible hypothesis in planning this study that there would be a significant positive correlation between degree of severity of the patient's disturbance and degree of difference in his value pattern from that of his spouse. A major negative result of this study is that no such relation was found between the *D*'s and the severity ratings, regardless of whether the 1, 2, 3 ratings, or the a, b, c ratings, or a combination of these was used. The same negative result was obtained when *D*'s were calculated between each patient's ratings of the Ways and the mean ratings of the Ways by the control group. As far, then, as this study goes, the degree of disturbance of the patient is not significantly correlated with the difference between his value pattern and his spouses's, or the difference between his value pattern and the average value pattern of his culture. If conflicts of value patterns are related to severity of mental illness, it seems that they must be sought elsewhere—as in the conflict within value patterns in the individual himself, and in the conflict between the individual himself and the particular persons with whom he has been in significant interaction in his life history. The first of these alternatives will be considered in the following section; the data do not permit consideration of the second alternative.

The severity ratings did, however, reveal some tendencies. Thus there was an interesting (though not statistically significant) negative correlation of .25 between the degree of severity and the score on Factor *A* (social restraint and self-control). An analogous result is the significant positive correlation (.38) between the *spouse's* score on Way 1 (preserve the best that man has attained) and Way 10 (control the self stoically) and the severity of the patient's disturbance. Since Way 1 and Way 10 are the high ranking Ways on Factor *A* (social restraint and self-control), we see again the importance of the values represented in Factor *A*. Not only are the patients as a whole distinguished from the controls by a lower score on this factor, but there is a tendency for the patient's disturbance to be more severe if he is low on this Factor—or if his spouse is high on it. To add to the same point, there is a positive correlation between egocentricity and degree of severity of disturbance. This may be because the value systems of the Ways are picking up

[10] The degree of severity rating scale used in this study was adapted from the "Clinical Rating Scale for Psychotic Patients" which was developed by Evelyn Crumpton, Ph.D., Psychology Staff, Brentwood Neuropsychiatric Hospital, Veterans Administration Center, Los Angeles 25, California.

TABLE 9

MEAN RATINGS OF THE WAYS BY THE LESS SEVERELY AND MORE SEVERELY DISTURBED PATIENTS

Way	Less Severely Disturbed	More Severely Disturbed
1	5.08	4.52
2	2.52	3.12
3	4.12	3.76
4	3.44	4.00
5	4.04	3.96
6	4.00	3.68
7	5.76	5.48
8	4.60	3.52
9	2.60	3.00
10	3.32	2.80
11	2.16	2.84
12	3.68	3.88
13	2.48	2.84

one of the major diagnostic features in the categories of mental illness—egocentricity or narcism.

A comparison of the ratings of the Ways of the less severe and the more severe personality disturbances brings out clearly a number of points that have been made. Table 9 serves as the basis for this comparison.

It will be noted that the more severely disturbed patients are lower in Ways 1, 3, 6, and 10 and higher in Way 4—and this combination defines Factor *A*. They are lower in Ways 1, 3, and 8 and higher in Ways 2 and 4—and this combination defines egocentricity. They are slightly lower in Factor *B* (enjoyment and progress through action) and considerably higher in Factor *C* (withdrawal and self-sufficiency). They are markedly lower on Way 8 (live with wholesome carefree enjoyment).

VALUE CONFLICTS AND SEVERITY OF ILLNESS

It has at times been suggested that conflicts of value within the individual are involved in some or all forms of mental illness. Our data permit some preliminary examination of this complex area, and suggest that such conflict is indeed found, though its relative importance cannot yet be estimated.

As a very rough measure of value conflict, the two Ways with heaviest loadings on the value factors *A*, *B*, *C*, *D*, and *E* were chosen.[11] These Ways were grouped into two sets. One set was composed of the more egocentric member of each pair of Ways, and the other set contained the less egocentric (and more socialized)[12] member. The determination was made in terms of the loadings of the Way on the previously mentioned egocentric-socialization factor. The more egocentric set (to be called *E*) contained Ways 10, 12, 2, 13, and 4 (representing respectively Factors *A*, *B*, *C*, *D*, *E*) and the more non-egocentric or socialized set (to be called *S*) contained Ways 1, 5, 11, 9, and 8 (representing respectively Factors *A*, *B*, *C*, *D*, *E*).

Since patients and groups of patients differed in the sum total ratings of the 13 Ways, rank orders of the ratings of the Ways were used rather than the ratings themselves. An *E* score, an *S* score, and a total score of *E* plus *S* was computed for each patient. The score for a set is simply the sum of the ranks given to the five Ways in the set. The lower the score the higher the value conflict. Thus if a person should like very much all the Ways in a set, there would be a conflict between all five value dimensions and the sum of the rankings would be small. A second (though related) value conflict score was obtained for each patient by counting the number of factors in a set which had ranks of 1 through 4.5. The Ways representing these ranks had also to be liked. (This did not hold for 3 cases, so the number of patients counted in this way was 47 instead of 50.) Such factors are said to be "in conflict." These measures of value conflicts leave much to be desired, but they do reveal matters of interest.

We turn first to the simple scores for the number of factors in conflict in the two sets *E* and *S*. The factors in conflict per person in the patients increase progressively as the intrapsychical severity increases

[11] Factor *B* is "progress and enjoyment through action"; Factor *C* is "withdrawal and self-sufficiency"; Factor *D* is "receptivity and sympathetic concern"; Factor *E* is "sensuous enjoyment."

[12] It will be recalled that the order of the Ways from egocentric to non-egocentric was 2, 4, 12, 6, 13, 11, 5, 10, 9, 7, 8, 1, 3.

TABLE 10

VALUE CONFLICTS AND DEGREE OF SEVERITY OF DISTURBANCE

$E + S$ Score	1's	2's	3's
72 or below	1	10	7
73 or above	12	15	5

(1.55 for the 1's; 2.18 for the 2's; 3.17 for the 3's). The average factors in conflict per person for the patients as a whole is 2.24, as contrasted to the average of 1.42 for the controls. Similarly, the percentage of persons who have factors in conflict in these three patient groups progressively increases (the corresponding figures are 46.2%, 82.6%, 100%). The percentage of persons with value factors in conflict is 79 for the patients and 64 for the controls. These figures are evidence that value conflicts are related to the degree of severity of the disturbance, and that there are more factors in conflict for the patients than for the controls.

As Table 10 shows, those higher in value conflicts (and so the low scorers) tend more to be 2's and 3's than 1's, while those lower in value conflict tend more to be 1's or 2's than to be 3's. The chi-square results are at the .03 level of significance.

If it be assumed that the groups 1b, 1c, 2a, 2b, 2c, 3a, 3b form a scale of increasing degrees of severity, then no important relation is found between the mean E or S scores of each patient group and its degree of severity. Nor between the E and S scores themselves. This shows that the grouping of the Ways in S and E is not sensitive enough to pick up the relation of egocentricity and severity of disturbance found

TABLE 11

VALUE CONFLICTS AND DEGREE OF SEVERITY OF DISTURBANCE

Group	$E + S$ Score
1 b	77.70
1 c	76.67
2 a	74.35
2 b	72.83
2 c	74.14
3 a	73.81
3 b	70.00

TABLE 12

AVERAGE SCORES FOR PATIENTS AND CONTROLS

	Average S	Average E	Average $E + S$
Controls	34.55	42.02	76.57
Patients	35.33	39.10	74.43

elsewhere in this study.[13] However, the situation is different with the sum of the E and S scores. This total value conflict score decreases (i.e., the value conflict is greater) as the degree of severity increases. The only exception is for the 2b group. Table 11 shows these results. The correlation between degree of severity and degree of value conflict is .89 and significant at the .01 level.

The patients had in 38.3 percent of the cases a smaller E than S score, while in the control group the figure was 20 percent. So for the controls there are fewer egocentric forms of value conflicts than for the patients.

The average score for E and S, and so the total of E plus S, is much the same for the patients and the controls. (See Table 12.) This we would expect since we know that the order in which the Ways are liked is very much the same in the two groups.

Nevertheless the distribution is quite different in the two cases. The patients have more low E plus S scores (and hence there is greater value conflict in these patients) than do the controls, and few high E plus S scores. The chi-square is 6.70, which is significant at the .01 level. Table 13 gives the data.

These results suggest that value conflicts

TABLE 13

DIFFERENCES BETWEEN HIGH AND LOW VALUE CONFLICT SCORES FOR PATIENTS AND CONTROLS

$E + S$ Scores	Controls (50)	Patients (50)
64–74	14	26
75–85	36	24

[13] Thus while Way 13 is slightly more to the egocentric end of the egocentric-non-egocentric scale it is not markedly egocentric. The same holds for Ways 10 and 1. See Note 12.

merit systematic investigation in the scientific study of mental illness. They of course say nothing about the origin of value conflicts, nor whether they play a causal role in the development and maintenance of mental illness. The origin may lie in the incorporation in the child of conflicting values of the parents, or in the conflict of parental values with the values encountered in other persons. There may also be biological elements at work, since some of the value factors (especially *B* and *C*) correlate to some degree with constitutional differences. (See Morris, 1956, p. 134–135.) The question of the causal role of value conflict in personality disturbance might be approached as part of the larger and difficult problem as to how symbols, and conflicts of symbols, operate in human behavior.

THE MARRIED PATIENTS

Of the 50 patients, 32 were married. Of these married patients, 16 of them were described in the original data from the Hacker Foundation as having marital problems. This group will be called *MP;* the group composed of the 16 other married patients will be called *OM*. It turns out that there are considerable differences between these two groups.

MP includes a preponderance of schizophrenics and schizoid trends, while *OM* is dominated by neurotics and passive dependent trends.

The rating of Way 3 (show sympathetic concern for others) sharply differentiates the two groups: the number of persons in *OM* who like Way 3 is much greater than the number of persons in *MP* (chi-square significant at the .001 level). Also *OM* favors Factor *B* (enjoyment and progress in action) much more than does *MP*, and favors Factor *A* (social restraint and self-control) much less. The high loadings on Factor *B* are for Way 12 (chance adventuresome deeds), Way 5 (act and enjoy life through group participation), and Way 6 (constantly master changing conditions). The high loadings on Factor *A* are for Way 1 (preserve the best that man has attained) and Way 10 (control the self stoically). In *OM*, 11 patients have a higher score on Factor *B* than on Factor *A*, while this is the case for only two persons in *MP* (chi-square significant beyond the .01 level).

OM also favors Factor *E* (sensuous enjoyment) with high loadings on Way 4 (festivity and solitude in alternation) and Way 8 (live with wholesome, carefree enjoyment), while *MP* is somewhat higher in its rankings on Factor *C*[14] (withdrawal and self-sufficiency) with high loadings on Way 2 (cultivate independence of persons and things) and Way 11 (meditate on the inner life).

OM in general likes the set of Ways to Live considerably more than does *MP* (the average sum of the ratings of the 13 Ways is 52.81 for *OM* and 44.05 for *MP*). It is interesting that the average sum of the ratings of the spouses and close friends corresponds closely with that of the patients; 53.63 in the first case and 44.94 in the second. *MP* in general has no enthusiasm for any of the Ways. *MP* used rating 7 only 12 times, and more than half of the group never used the rating at all. *OM* used rating 7 thirty times. The lowest rating, 1, was used 29 times by *OM* but 41 times by *MP*.

From all these data, a definite picture emerges. Those within this group of patients with specifically mentioned marital problems are more constrained, more detached, and more conservative than the other married patients. They show less zest, less receptivity, and less capacity for enjoyment. They are higher on Factor *A* (social restraint and self control) and Factor *C* (withdrawal and self-sufficiency) than the other married patients, and lower than them on Factor *B* (enjoyment and progress in action) and Factor *E* (sensuous enjoyment).

In the control group, and in the Americans sampled in *Varieties of Human Value* (Morris, 1956), Factors *A*, *B*, and *E* are all of substantial weight—social restraint, activity, and enjoyment are all at work. In the married patient groups, however, an interesting split in these factors occurs. In the married subjects with distinct marital problems, Factor *A* remains relatively high, but Factors *B* and *E* are low, while in the

[14] When corrections are made for the generally lower ratings of MP.

other married subjects the situation is reversed: Factor *A* has become low in strength, and Factors *B* and *E* have become relatively much stronger. Those with noticeable marital problems value, as it were, an excess of restraint over action and enjoyment, while the other married patients value an excess of action and enjoyment over restraint. Thus the married patients as a whole deviate in these two divergent directions from those who are not patients.

USE OF THE RATING CATEGORIES 1 THROUGH 7

The most characteristic way in which patients differed from controls in using the rating categories was in their greater use of dislike and indifferent ratings (1, 2, 3, 4) and their avoidance of favorable ratings (5, 6, 7). As seen in Table 14, this difference is consistent all along the scale. As estimated by the Kolmogorov-Smirnov method, chi-square is 7.39, significant at the .05 level.

This difference of patients and controls might be interpreted in two ways—either as indicating a difference in the meaning of the categories to the two groups (so that, say "like quite a lot" for the patients means about what "like very much" does for the controls), or as showing a greater disliking of the Ways by the patients. We shall adopt the second alternative; a number of considerations seem to warrant this choice. (See, for instance, Morris, 1956, p. 121.)

If this alternative is adopted, then almost half of the patients found no Way to Live among the 13 that they liked very much. This is to say that they never used category 7 of the rating scale. Most of these persons were either schizophrenics or showed schizoid trends. This is perhaps related to the low amount of emotional investment such persons show in persons and things outside themselves. In general they were functioning poorly in their work and in their marriages. There is a great deal of overlap in the groups listed in Table 15, with schizoid trends as the common theme. The difference between the use or non-use of category 7 in the schizophrenics and their controls is significant at the .001 level (chi-square of 13.3; $N = 24$).

Not all schizophrenics failed to use the rating 7. Of the twelve schizophrenics, ten used 7 not at all, one used 7 once, and the twelfth used 7 six times. The only acute schizophrenic in this study used the rating 4 (indifference) ten times, while his wife used it six times. Thus, of twenty-six ratings, sixteen were indifferent. One paranoid schizophrenic used 1 (dislike very much) seven times, 6 (like quite a bit) five

TABLE 14

DISTRIBUTION OF RATINGS

Rating	Controls	Patients
7	111	78
6	115	107
5	97	89
4	75	88
3	118	130
2	80	102
1	54	56
Total	650	650

TABLE 15

PERCENTAGE OF DIFFERENT GROUPS WHO GAVE NO 7 (LIKE VERY MUCH) RATINGS TO ANY WAY

Group	*N*	Percentage
Control	50	28
Patients	50	44
All patients except most severe	38	32
Most severe patients (all schizophrenics)	12	83
Spouses and friends of most severe patients	12	58
Work problems	13	77
Marital problems	16	56
Other married	16	31

TABLE 16

NUMBER OF RATING CATEGORIES USED (OUT OF 7) BY AVERAGE MEMBERS OF DIFFERENT GROUPS

Group	*N*	$\bar{X}$
Control	50	6.00
Patients	50	5.40
Spouses and friends	50	5.20
Schizophrenics	12	4.67
Spouses and friends (of schizophrenics)	12	4.92
Work problems	13	4.54

TABLE 17
SUCCESSIVE INTERVAL SCALE VALUES OF UPPER CATEGORY LIMITS

Scale	*N*	Rating Category					
		1	2	3	4	5	6
Patients	50	−1.238	−.593	−.168	.168	.731	1.353
Controls	50	−1.343	−.665	−.205	.205	.817	1.646
USA Male college students	252	−1.417	−.709	−.216	.216	.872	1.701

times, and 4 (indifference) once. Every schizophrenic in this study used two of the seven available categories to rate a majority of the Ways. Two-thirds of the schizophrenics used only two categories to rate nine or more Ways, a phenomenon not found at all among the least severe patients and found in less than a third of the less severe patients. Measuring the use of rating categories by taking the number of categories used more than once differentiates between the most severe and the least severe patients at the .001 level of significance (chi-square of 15, *N* of 24, *df* of 1).

What seems to be happening is that the lack of freedom and the inhibitions found in the schizophrenic patient expresses itself in their inability to shift in their discriminations and perceptions. The tendency of schizophrenics to use fewer rating categories has already been reported by Charles E. Osgood (See Osgood, Suci, & Tannenbaum 1957, p. 227).

The tendency to use fewer categories is, however, not limited to schizophrenics, nor to the patients only. The *t* of 3.3 between the average number of categories used by the patients and the average number used by the controls is significant at the .01 level, but the same comparison between controls and the spouses and friends of patients ($t = 4.5$) is significant at the .001 level. Averages are listed in Table 16. Use of all like, dislike, and indifference categories by all members of a group would result, of course, in an average of 7.00; so that the control group average of 6.00 means that the average control person used all but one of the seven.

The question may now be raised whether the rating categories as used by the patients can be treated as integers, i.e., whether they form a scale which permits making correlations, computing means, carrying out factor analyses, and the like. This was earlier found to be the case with samples of 1546 and 252 male college students (Morris, 1956, pp. 20–27). The analysis of the ratings of the present control group gives results highly similar to the two previous analyses.

With the patients, however, an interesting difference occurs; the intervals between the rating categories all become smaller, i.e., the whole scale contracts.[15] This contraction of the scale is further evidence that the patients are a much less homogenous group than the controls. It is important to realize this, for to lump all the patients together in one group obscures some of the similarities between neurotics and controls, and obscures the differences between the diagnostic sub-groups. Nevertheless, it is important to note that the patients are using the seven response categories as a scale, even though it is a contracted scale when compared to the controls (or to the previously studied groups of male college students). The correlation between the scale values of the upper category limits and the original ratings interpreted as integers is .990. It is thus legitimate to treat the rating categories of the patients (though differing in important respects from those of the controls) as if they were integers. This justifies the use of these rating categories for making the product-moment correlations used in factor analyses. Table 17 compares a group of 252 male college students, the present control group of 50 persons, and the 50 patients with respect to the upper limits

[15] The method used is found in Guilford (1954, pp. 237–241). This method does not permit the determination of the lower boundary of 1 or the upper boundary of 7.

of the first six rating categories. The results are expressed cumulatively and as positive or negative upon the convention that the middle point of the original response category 4 is the zero point.

DISCUSSION

The results of this exploratory study seem to justify the belief which initiated it, namely, that an approach to personality disturbances through a study of values bears promise, and that such a study can fruitfully employ quantitative techniques. The present study, however, mainly indicates a direction of possible research. Instruments other than the "Ways to Live" will be needed, and the study of groups will need to be supplemented by the intensive study of individuals. It is possible, in line with a common trend in psychology, that the study of value patterns in emotionally disturbed persons may provide clues as to how such value patterns function in normal and above-normal persons.

One positive result of the present study is that in general the group of patients rank the Ways in about the same manner as does the control group, and Americans at large. Their conceptions of the good life are not in general different from those found in other members of the culture in which they live.

Furthermore, the data disclose no general relation between the degree of severity of personality disturbance and the degree of difference of the value pattern of the disturbed person from that of his culture or of his spouse or friend.

Nor does it seem possible to say that the person who is emotionally disturbed is simply not living in accordance with his conception of the good life. The general similarity between the favored Ways and actual behavior is attested to by the fact that the analysis of patient sub-groups in terms of the Ways is largely in agreement with accepted clinical observation of the patients' behavior—a point which will be developed at length somewhat later.

It seems rather as if the "pathic" ways to live are distorted, inefficient, and hence unsatisfying versions of the same ways to live found in healthy persons.[16] Psychotherapy, for quite independent reasons, aims at what this suggests: not the changing of the values of the disturbed person but helping him develop more effective techniques of realizing those values he holds.

The data do show evidence linking the degree of severity of the disturbance with the amount of conflict among the values held by the disturbed person. It does not follow that conflicts among values necessarily lead to difficulties which call for psychotherapy—for many persons without such disturbances have as many, or more, value conflicts as do the patients. For one reason or other, the patient is unable to find efficient ways of managing such conflicts, which thereby become anxiety producing and incapacitating.

There are indeed interesting differences in the value patterns of sub-groups of patients, and these in general correspond to differences in behavior as clinically observed. But differences of the same (or greater) magnitude are found between normal sub-groups in our culture. Thus it is not the differences as such between the sub-groups of patients which account for their different symptoms or syndromes. Nor do our data permit any speculations about the etiology of the various kinds of personality disturbances. At best they show that the values held have some relationship to the kind of personality disturbance that develops.

Though the preceding remarks indicate the main direction of this study, they must not be taken to mean that there are no value differences between the patients and controls, nor between the various sub-groups of patients themselves. We have noted such differences in the use of the rating categories, in the mean ratings of a number of Ways (as opposed to the order of rankings), and in the patients' favoring of egocentric Ways more than do the controls. The most disturbed and the least disturbed patients showed clear differences, as did those patients who were having severe marital problems. These differences are as

[16] See Morris, (1948, pp. 114–116) for an earlier speculative presentation of this position.

important as the general similarities previously noted.

It should be stressed that all the results obtained and reported were derived solely from the summary data furnished by the Hacker Clinic Associates—no other information concerning the patients was used. Hence the statistical analysis was in a sense "blind." Those making it had no contact whatever with the patients themselves. It is of considerable interest, therefore, that the results picked up from the single "Ways to Live" instrument correspond so well with clinical observations. The ratings of the Ways provide an expression of some of the characterological features of the psychiatric patients that are commonly a part of the diagnostic picture they present. The ratings of the Ways seem to present a sample of behavior which accords well with other diagnostic features which characteristically distinguish the various nosological groups.

For example, the obsessive-compulsives, seen in terms of the ratings of the Ways, show little interest in extroverted activity or sensuous enjoyment, but rather stress self-control, a turn toward intellectual matters, and emotional withdrawal—all characteristics which pathognomically identify them. Similarly, the ratings of the Ways by the hysterics fit in with the clinical picture of their stress on sensuality and enjoyment, and their high dependency which influences both their emotional and intellectual development. As a further example, the ratings of the paranoid schizophrenics seem to show a strong intellectual tendency which is in line with their propensity for ideational thinking (which usually assumes delusional proportions), and their caution and overcontrol (which usually is related to their suspiciousness). This is in contrast to the picture of the non-paranoid schizophrenics which their ratings give, and which fits in with the clinical observations that such cases do not stress the intellectual developments to which paranoids are prone, and are often much more labile in their affective behavior.

It seems possible that this kind of diagnostic confirmation might be extended in further study by comparing how much certain groups of patients project themselves in their ratings of the Ways as compared with other patient groups. Thus it may be that for the paranoid patients the Ways are more ideals than revelations of how they actually live. Also the high value given to Ways which stress control by the schizophrenics may be in part a projection of what they feel is a lack in their own personality. For some of the other groups, such as the more neurotic ones, there seems to be in their ratings less projection and more actual description of their personality patterns in other areas of behavior. An investigation into such problems would disclose how far in various groups conceptions of the good life (conceived values) are consonant with, or divergent from, actual behavior (operative values).[17] It would also be interesting to study the relation of the discrepancy between conceived and operative values to the degree of severity of personality disturbances.

In this study, not only the content of the Ways rated but certain formal or structural characteristics of the ratings (flexibility in ratings, their consistency, whether constantly high or low, etc.) have also been considered. Such characteristics reflect certain aspects of the personality, something of its "style," which would be missed if only the content of the Ways was under consideration. This use of both structural characteristics of response as well as content categories is similar to methods used in the Rorschach and other projective tests.

Since the patient sub-groups here studied are very small, it would be desirable to administer the Ways (and other value instruments) to larger carefully diagnosed groups in order to confirm or qualify these first findings.

REFERENCES

Guilford, J. P. *Psychometric methods.* (2nd ed.) New York: McGraw-Hill, 1954.

[17] For an elaboration of the distinction between conceived and operative values, see Morris, 1956, pp. 10 and 11.

Horney, Karen. *Our inner conflicts*. New York: W. W. Norton, 1945.

Horney, Karen. *Neuroses and human growth*. New York: W. W. Norton, 1950.

Morris, Charles. *The open self*. New York: Prentice-Hall, Inc., 1948.

Morris, Charles. *Varieties of human value*. Chicago: Univ. Chicago Press, 1956.

Osgood, C. E. & Suci, G. J. A measure of relation determined by both mean difference and profile information. *Psychol. Bull.*, 1952, 49, 251–262.

Osgood, C. E., Suci, G. J., & Tannenbaum, P. H. *The measurement of meaning*. Urbana: Univ. of Illinois Press, 1957.

(Manuscript received February 15, 1960.)

The intellectual is constantly betrayed by his own vanity. God-like, he blandly assumes that he can express everything in words; whereas the things one loves, lives, and dies for are not, in the last analysis, completely expressible in words. To write or to speak is almost inevitably to lie a little. It is an attempt to clothe an intangible in a tangible form; to compress an immeasurable into a mold. And in the act of compression, how Truth is mangled and torn! The writer is the eternal Procrustes who must fit his unhappy guests, his ideas, to his set bed of words. And in the process, it is inevitable that the ideas have their legs chopped off, or pulled out of joint, in order to fit the rigid frame.

ANNE MORROW LINDBERGH, *The Wave of the Future*

What factors would the owners of business firms—whether they are individual owners of corporate stockholders—take into account in selecting a business manager to produce the kind of company they are interested in having? This study shows, contrary to previous economic theories based on profit maximization, that depending on how closely the owners are involved with the firm, methods of compensation to a manager and his internal motivations are deciding factors in a firm's success.

THE MOTIVATION OF THE BUSINESS MANAGER

by Lawrence A. Kratz

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THE business manager is the most important decision maker in the American economy. To a very large extent, the business manager decides whether America shall have prosperity or depression, inflation or deflation, progress or stagnation. The decisions of the business manager are the product of two factors: the outer environment surrounding him, and the inner motivations governing him.

Business analysts have painstakingly scrutinized the outer environment of the business manager, but have paid remarkably little attention to his inner motivation. Most business analysts are content to assume that the prime, dominant, and controlling purpose of all, or nearly all, business managers is that of maximizing the total profit of the business firm. Operating on this assumption, business analysts have erected an elaborate framework of economic theory which leads them to the following conclusions: (1) that business managers will increase the physical output of the business firm in response to: (a) an increase in the price of the firm's product arising from stronger demand for it; (b) a reduction in taxes upon the firm; or, (c) a fall in the firm's costs of operation; and (2) that business managers will curtail the physical output of the business firm in response to the opposites of the above circumstances. These conclusions may logically follow from the profit-maximizing assumption. But reality sometimes departs from that assumption. Some business managers do, and others do not, strive to maximize the total profit of the business firm. Whether any particular managerial staff is or is not a profit-maximizing team largely depends upon: (1) whether the owners of the business firm closely supervise the managerial staff; (2) the method of compensating the managerial staff of a business firm; (3) the personality, temperament, and education of the managerial staff; and (4) other factors not considered in this article.

Business firms may be divided into two broad categories on the basis of the locus of control: owner-dominated firms and manager-dominated firms. The owners of most business firms are single-mindedly and whole-heartedly dedicated to the total profit-maximizing ideal. When the owners dominate a firm, its managers look upon profit-maximizing activity as job insurance. However, in many large corporations, the common stockholders cannot or will not exercise control. The impotence of the common stockholders arises from fragmentation of ownership, ignorance, and apathy. By default, such corporations have become manager dominated. When a firm is manager dominated, the managerial staff may or may not seek to maximize total profit, depending on: method of compensation; and personal, temperamental, and educational factors.

On the basis of method of compensating management, manager-dominated business firms may be divided into three categories: (1) Straight-Salary Firms, wherein the

managers receive compensation which is fixed as to absolute amount; (2) Straight-Commission Firms, wherein the managers receive compensation which is a fixed percentage of total profit; and (3) Mixed-Compensation Firms, wherein the managers receive compensation containing elements of salary and commission.

In a manager-dominated straight-salary firm, the managerial team is unlikely to be vitally concerned about maximizing total profit. In such a firm, management hopes for no reward if profit is maximized, and fears no punishment if profit is not maximized. However, the managerial team is very definitely interested in the making of essential outlays on time and in full. By essential outlays is meant all money payments which must be made by a business firm if the managerial staff is to have job security and satisfactory compensation. Essential outlays include the following items: (1) Fixed Costs, such as real estate taxes or rent, bond interest, and so forth; (2) Variable Costs, such as wages of labor and prices of raw materials; (3) Complacency Profits, i.e., total profits just sufficient to preserve owner lethargy; and (4) Managerial Compensation, i.e., the straight salaries of the managers.

If Fixed and Variable Costs are not paid promptly and in full, financial complications may jeopardize the job security of management. If a Complacency Profit is not paid to inert owners of the firm, they may become active, hostile, and rebellious.

The Total Outlay of a business firm is the sum of all essential outlays (i.e., Total Fixed Cost, Total Variable Cost, Complacency Profits, and Managerial Compensation). The Total Outlay cannot be paid unless Total Revenue (i.e., total income from sales of physical output) is no smaller than Total Outlay. Consequently, the managerial staff is deeply concerned about securing Total Revenue no smaller than Total Outlay.

There is a certain level of physical output at which the Total Revenue of a business firm just equals its Total Outlay. This output level may be called the Break-Even Point. At output levels lower than the Break-Even Point, Total Outlay exceeds Total Revenue.

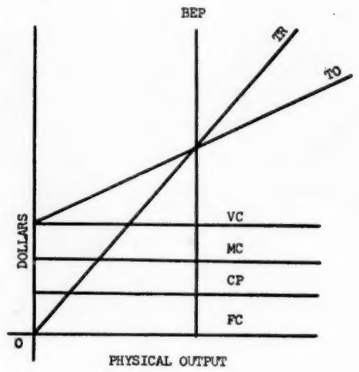

FIGURE 1

However, at output levels higher than the Break-Even Point, Total Revenue may exceed Total Outlay. In a manager-dominated straight-salary firm, it is clearly in the interest of both owners and managers for the output level to be lifted up to the Break-Even Point. But the interests of owners and managers diverge if the output level is lifted above the Break-Even Point. By raising output above the Break-Even Point, management would not only tend to maximize the total profit received by the firm's owners, but would also tend to increase the problems, responsibilities, and work load of the managerial staff without increasing its pay. This assertion is based on the premise that there is a crude but significant relationship between the size of the physical output of the business firm on the one hand, and the problems, responsibilities, and work load

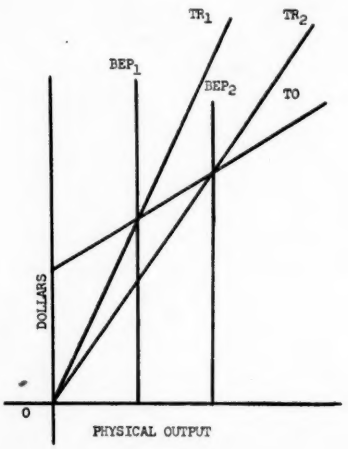

FIGURE 2

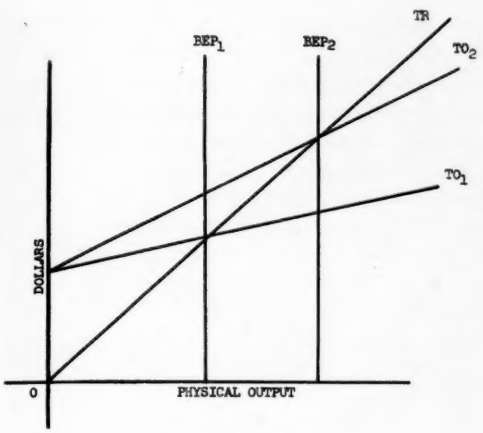

FIGURE 3

of management on the other hand. There is an almost universal proclivity to resent and resist extra exertion unaccompanied by extra pay. Therefore, it is to the interest of the managerial staff to lift physical output up to but not above the Break-Even Point in a manager-dominated straight-salary firm.

The determinants of the level of physical output in a manager-dominated straight-salary firm are illustrated in Figure 1. Lines *TR* and *TO* represent Total Revenue and Total Outlay at various levels of Physical Output. Line *BEP* represents the Break-Even Point, i.e., the only output level at which Total Revenue just equals Total Outlay. The triangular *VC* layer represents Variable Costs. The rectangular *MC*, *CP*, and *FC* layers represent Managerial Compensation, Complacency Profits, and Fixed Costs respectively.

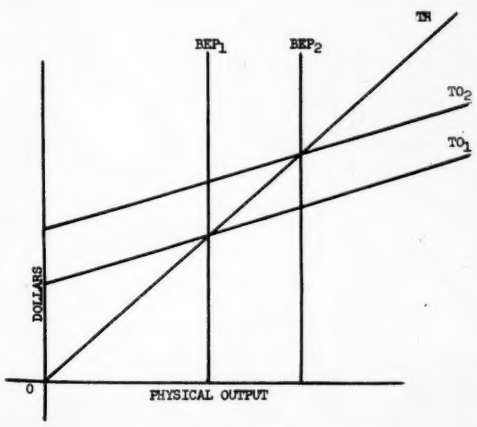

FIGURE 4

If the managerial staff of a manager-dominated straight-salary firm endeavors to raise or lower the output level of the firm to the Break-Even Point, it follows that any circumstance which alters the Break-Even Point will change the equilibrium output level of the firm. Changes in Total Revenue, Variable Cost, and Fixed Cost alter the Break-Even Point. In Figure 2, a fall in the price of physical output connected with weakening demand moves the Total Revenue line to the right. In Figure 3, an increase in a Variable Cost, such as the wage of labor, moves the Total Outlay line upward. In Figure 4, an increase in a Fixed Cost, such as real estate rent or taxes, moves the Total Outlay line upward. Figures 2, 3, and 4 share a vital characteristic. In each figure, the Break-Even Point moves to the right, thereby motivating the managerial team to increase physical output. Hence, in a manager-dominated straight-salary firm, a price reduction, a wage increase, or a rise in real estate rent or taxes can bring about an expansion of physical output contrary to the expectations of conventional economic theory.

To simplify these diagrams, it has been assumed that the firm can market any amount of output at the same price, and that average variable cost is the same at all output levels. Had other assumptions been made with respect to prices or costs, the diagrams would have been more complicated, but the conclusions reached would have been much the same.

In a manager-dominated straight-commission firm, the managerial staff may or may not be oriented toward maximizing total profit for the business firm. The deciding factor is the personality, temperament, and education of the managers. Some business managers seem to have an infinite and insatiable craving for money, power, and prestige, while others have decidedly limited money, power, and prestige objectives. The former may be referred to as narrow-interest managers, and the latter as broad-interest managers. The narrow-interest manager is preoccupied with the pursuit of money, power, and prestige because he has relatively

little interest in other values such as family life, the fine arts, literature, social activity, and so forth. At the other extreme, the broad-interest manager has modest, limited, specific goals as regards money, power, and prestige because he wants to reserve a large part of his time and energy for the cultivation of other values. If a straight-commission firm is dominated by a narrow-interest managerial staff, both owners and managers will rank total profit maximizing well above other objectives. However, if the straight-commission firm is dominated by broad-interest managers, the motivation of the managerial team will be the same as in a manager-dominated straight-salary firm. The salaries of broad-interest managers are fixed by their own limited, modest, specific income objectives. After certain managerial income levels have been attained, broad-interest managers refuse to invest more of their time and energy in the pursuit of money, power, and prestige at the expense of alternative values. Hence, the Break-Even Point analysis which applies to manager-dominated straight-salary firms also applies to broad-interest, manager-dominated, straight-commission firms.

As a result of grave deficiencies in psycho-economic data, it is presently impossible to ascertain the relative importances of: (1) owner- and manager-dominated firms, and (2) narrow- and broad-interest managers. However, two trends appear to be fairly clear: the increasing size and complexity of the typical business firm discourages owner domination; and the increasing emphasis on higher education for management candidates, together with the liberalization of the business college curriculum, favor the development of broad-interest managers. These trends have profound implications for economic theory, corporation security analysis, and public policy.

(Manuscript received November 10, 1959)

There were three baseball umpires who were discussing their profession. The first one said, "Some's balls and some's strikes and I calls 'em as they is." The second one said, "Some's balls and some's strikes and I calls 'em as I sees 'em." While the third one said, "Some's balls and some's strikes and they ain't nothing till I calls 'em."

CANTRIL and BUMSTEAD, *Reflections on the Human Venture*

How do intelligent individuals actually behave in the conflicts of games of strategy? Do they behave as the game theory models require? Here is an experiment which attempts to relate observed behavior in a game situation with what the theory of games says about such behavior. Such experimental work is another step toward the construction of adequate descriptive theories of conflict behavior.

HUMAN BEHAVIOR IN A STRICTLY DETERMINED 3 × 3 MATRIX GAME

by Bernhardt Lieberman[1, 2]

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THE purpose of this study was to determine how closely the behavior of a group of intelligent individuals conformed to the commands of the minimax model when playing a two-person, zero-sum game having a saddle point. The study indicates how well one element of the theory of games describes and predicts the behavior of a certain group of intelligent individuals.

Game theory, in its entirety, is a complex, mathematical theory of conflict of interest and social interaction. It consists of a number of individual models dealing with numerous situations including two, three, and *n*-person games that may be zero-sum or non-zero-sum. The totality of theoretical considerations involved in all these situations is labelled the theory of games of strategy. The minimax model, utilized in this study, is one such element of the theory. The model is normative; when two intelligent persons are in conflict for quantifiable objects, it prescribes proper choices for the opponents if they wish to achieve certain conservative ends. The model does not predict that people actually do behave as it commands. It would be particularly interesting if intelligent individuals did behave as the model prescribes and it is also a matter of independent interest to determine how people actually do behave in such situations.

Previous discussions of human behavior concluded that people do not behave as the minimax model states they should. The point of view expressed is that the minimax solution of two-person games recommends choices and is not a behavior theory. It does not describe the way an organism adjusts his behavior to the environment (Estes, 1957; Suppes and Atkinson, 1959). Little experimental work has been done to decide what is essentially an empirical question.

A small number of empirical studies of human behavior, in game-like situations, had been done prior to the current work (Flood, 1952; Estes, 1957; Vinacke and Arkoff, 1957; Atkinson and Suppes, 1958, 1959). The results of these studies did not indicate that people behave as the game models prescribe. However, each of the studies done could not be considered a direct or adequate test of the predictive power of one of the game models; either the assumptions of the theory were not satisfied or the primary purpose of each study was other than a conclusive test of one of the game models.

In a very simple situation, a 2 × 2 matrix game with a saddle point, Lieberman (1959) found that ten of fourteen college students adopted a minimax strategy. The remaining four *S*s exhibited behavior similar to, but not identical with, the optimal behavior prescribed by the model. The

[1] This study was done during my tenure as a Public Health Service Postdoctoral Research Fellow of the National Institute of Mental Health.

[2] I wish to thank Professor R. Duncan Luce for his substantive contributions to this study. Discussions with Professor Frederick Mosteller helped to clarify my ideas.

results obtained in this study could not be considered convincing support for the model; the task facing the players was too simple. In the present study, an attempt was made to design a strictly determined game that was not immediately transparent and in this situation determine whether *S*s adopt minimax strategies; a 3 × 3 matrix game was chosen.

METHOD

The game. The *S*s played the following rectangular game.

		Player Red 1	2	3	row minima
Player Blue	1	+15	0	−2	−2
	2	0	−15	−1	−15
	3	+1	+2	0	0
column maxima		+15	+2	0	

In the above game the maximum of the row minima equals the minimum of the column maxima; the game has a saddle point. The value of the game is zero and the minimax model dictates that both players choose strategy 3, 100% of the time. For each player the optimal strategy does not dominate both nonoptimal strategies and for each, the optimal strategy has an average value less than one of the nonoptimal strategies. A certain symmetry is present in the above matrix; both players have essentially the same choices.

Subjects. The *S*s of the study were 30 men, undergraduate students of Harvard College in residence during the academic year 1958–1959.

Materials. The materials used consisted of the following items: (1) a matrix game board that identified the payoffs to the *S*s, (2) five dollars in cash for each game, (3) red, white, and blue chips, (4) red, white, and blue choice cards, (5) written instruction sheets, (6) pencils and scrap paper.

Procedure. Two *S*s played each game; each was assigned, by a flip of a coin, to a position as player red or blue. Each *S* was given $2.50 at the start of the game and with this he purchased $2.00 worth of chips from *E*. The *S*s played the matrix game 200 times. Each *S* held a number of his colored (red or blue) choice cards in his hands. A play of the game, a trial in the experiment, consisted of each *S* selecting a choice card with either a 1, 2, or 3 on it, placing it face down on the table, and turning it over simultaneously with his opponent, revealing the choices. *S*s exchanged chips after each play of the game and after the 200 choices were completed each *S* received an amount of cash from *E* equal to the value of the chips remaining in his possession. Payments were in cents; +15 indicates a 15-cent payment from red to blue and −15 indicates a payment from blue to red.

Blank white cards were available to the *S*s if they wished to submit grand strategies. If *S* believed he had a systematic plan of playing the game, no matter what his opponent did, he could write the plan on a blank white card and give it to *E*. This plan had to dictate what choice *S* would make on every remaining trial. *S* then had to play as his grand strategy dictated. If both *S*s submitted grand strategy cards the outcome of every succeeding play of the game was determined. When this occurred, the *S*s made their choices in accordance with the grand strategies for 20 plays of the game. If after the 20 trials both decided that they wished to continue playing as their plans dictated, they no longer made choices and the remaining payoffs were made as the two grand strategies dictated. Since, in the study, this occurred only when both *S*s adopted the minimax strategies, the games were terminated; the remaining payoffs were all zero. The use of grand strategies was adopted to prevent boredom from being a major factor in the *S*s' choices.[3]

The *S*s were not allowed to keep an accurate account of their winnings and losses but they were permitted to keep a record of their own and their opponent's choices; few did this.

Interview. Immediately after each game both *S*s were asked how they played the

game; an attempt was made to have them describe what factors affected their choices.

RESULTS

Of the total of 30 *Ss*, 12 submitted grand strategy cards by the 55th trial, indicating that all their future choices would conform to the behavior prescribed by the minimax solution. In addition, three other *Ss* chose strategy 3, 100% of the time over the last 75 trials.[3] If these three *Ss* are considered to have adopted the optimal strategy, then 15 or half of the *Ss* can be said to have conformed to the prescription of the minimax model. The remaining 15 *Ss* continued to make a number of nonoptimal choices throughout the 200 plays of the game.

Table 1 shows the mean percentages of choice of strategies, in blocks of 10 trials, for all *Ss*. On the first block of trials, the *Ss* designated red players chose the optimal strategy 66.7% of the time and those designated blue players chose strategy 3, 72% of the time. On the final blocks of trials the percentages were 92.7, 97.3 and 93.3 for the red players and 90.0, 93.3 and 92.0 for the blue players.

The data of Table 1 indicate that for both groups of players optimal choices rose from their initial level to be more than 90% on the concluding trials. It is clear that the choices of the optimal strategy did not reach the 100% level, the prediction of the minimax model. A small number of choices of the dominated strategies (*R*1 and *B*2) were made over the 200 trials. These appear to be scattered throughout the 200 plays of the game. Strategies *B*1 and *R*2 had higher average values, and were not dominated by *R*3 and *B*3. They were chosen approximately 30% of the time on the initial trials and choices of them decreased until they were being chosen only as often as the dominated strategies.

Given the results above it appears possible to distinguish between two types

[3] The last nonoptimal choice for these three *Ss* was made on trials 25, 32, and 125. The behavior of these *Ss* appeared to be quite different from the other 15 who did not submit grand strategy cards. The 15 would make 10 to 20 choices of strategy 3 but then would revert to one or more nonoptimal choices.

TABLE 1

PERCENTAGES OF CHOICE OF STRATEGIES OF THE PLAYERS

Plays-Trials	Red			Blue		
	1	2	3	1	2	3
1–10	3.3	30.0	66.7	26.0	2.0	72.0
11–20	1.3	22.7	76.0	16.7	2.7	80.6
21–30	0.7	16.0	83.3	11.3	0.7	88.0
31–40	2.7	12.0	85.3	9.3	2.0	88.7
41–50	2.0	7.3	90.7	6.0	3.3	90.7
51–60	2.0	12.0	86.0	6.0	6.7	87.3
61–70	2.0	11.3	86.7	6.0	8.7	85.3
71–80	2.0	4.7	93.3	6.0	3.3	90.7
81–90	1.3	6.7	92.0	4.7	6.0	89.3
91–100	1.3	8.0	90.7	2.7	5.3	92.0
101–110	2.0	4.7	93.3	5.3	4.0	90.7
111–120	2.0	4.7	93.3	2.7	5.3	92.0
121–130	4.0	4.0	92.0	4.0	3.3	92.7
131–140	0.0	4.7	95.3	3.3	4.0	92.7
141–150	4.0	6.0	90.0	5.3	3.4	91.3
151–160	2.7	5.3	92.0	5.3	4.7	90.0
161–170	1.3	6.0	92.7	2.7	2.7	94.6
171–180	4.0	3.3	92.7	5.3	4.7	90.0
181–190	0.7	2.0	97.3	2.7	4.0	93.3
191–200	2.0	4.7	93.3	4.0	4.0	92.0

of *Ss*, those who adopted the minimax strategy and those who did not. The data for those *Ss* who did could be represented by curves (of the percentage of optimal choice) which rise to the 100% level by the 125th trial and remain at this level. The behavior of the 15 *Ss* who are not considered to have adopted the minimax solution is presented in Figure 1.[4] On the initial trials, these *Ss* chose the optimal strategy 80.0 and 73.8% of the time, red and blue respectively. On the final 40 trials, *R*3 was chosen 87.3 and *B*3 was chosen 86.0% of the time. The choices of the 15 *Ss* who were rational would, of course, be 100% over the last 75 trials. The functions of Figure 1 indicate that the behavior of these *Ss* was changing over the 200 trials; a stable level of response was not reached. In addition, the curves suggest that the changes in behavior might be a nonlinear function of the number of trials.

The results indicate that the more often one player chose strategy 3 the more often his opponent did. As choices of *R*3 approach the 100% level in Table 1, *B*3 makes a

[4] Seven of these were players designated red and eight designated blue.

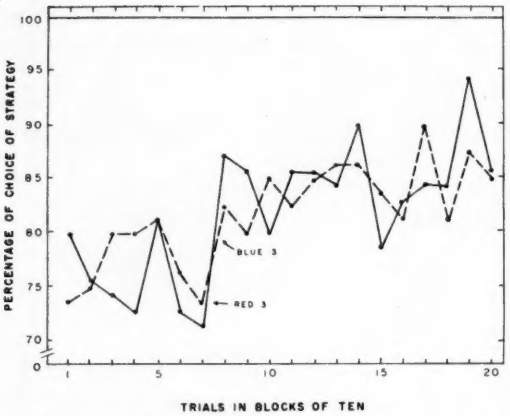

FIG. 1. Percentages of choice of the optimal strategy by players who did not obey the commands of the minimax model.

similar approach. *Ss* described this finding when, in the interviews, they made such statements as, "When I saw he was going to stick to 3, I had to." Similarly, when a player made nonoptimal choices his opponent was more likely to do the same. A number of *Ss* were "enticed away" from their successive choices of the optimal strategy by opponents who, though inviting loss, behaved nonrationally.

What occurred was the following. A blue player was regularly choosing *B*3 against an opponent who was repeatedly choosing *R*3. If the *Ss* did not submit grand strategy cards, after a number of occurrences of *R*3-*B*3 the red player might occasionally make a choice of *R*2, risking the loss of 2¢. Or, he might occasionally choose his dominated strategy (*R*1) risking the loss of 15¢ or 1¢. By this behavior he tried to encourage his opponent blue to choose *B*1 or *B*2. Red would win 15¢ if *B*2-*R*2 occurred, win 1¢ if *R*3-*B*2 occurred, or win 2¢ if *R*3-*B*1 occurred. Similar tactics were tried by some blue players against opponents who consistently chose *R*3.

Ss stated, in the post-experimental session interviews, that they made nonoptimal choices "to make the game interesting," or to attempt to win rather than neither win nor lose. There were players who did not deviate from making optimal choices even when their opponent did. These players repeatedly won small amounts of money (1¢ and 2¢) until their opponent ceased making nonoptimal choices. Some *Ss* persisted in making nonoptimal choices even when they failed to induce their opponent to abandon his rational behavior and when they stated that their own behavior was resulting in losses for themselves.

From observations of the *Ss* and the interviews obtained after the games were played, it was obvious that virtually all of the players, after a small number of trials, comprehended the essential elements of the matrix game. The *Ss* stated that a choice of strategy 3 was a safe, conservative way of playing, one which assured the player of no loss and offered the possibility of gain. Many *Ss* stated that one of their possible choices (the dominated strategy) should never be chosen and another possible choice (the one that offered +15, 0, −2) offered the opportunity of a larger win. The following are typical comments that were made. "I couldn't lose if I played 3; I could only win or break even." "At first I decided the only rational way to play would be to play 3." "One must always choose 3 if opponent is rational; then one breaks even."

Although the *Ss*' analyses of the conflict situation were accurate, the results reported above indicate that there were a significant number of choices of nonoptimal strategies. The *Ss* "reasons" for these choices are of some interest. The explanations offered ranged from boredom, a desire to please the experimenter, to just a wish to "play around." Typical comments were: "I should give him a chance to win some money." "When we got into a rut, I tried to get him to go for the 15¢." "I was too bored to play all threes." "If we both played 3, nothing would happen." "I played 1 just to be perverse." "If I played 3, that wouldn't make it interesting for me or for you" ("you" being *E*).

DISCUSSION

The task facing the *Ss* appeared to them to be a simple competitive game in which by selecting a particular course of action they could assure themselves of not losing and possibly winning small amounts of money. However, the particular matrix

game played was symmetric and for each player the game had an optimal strategy that did not dominate both alternate strategies. In addition, the game contained one nonoptimal strategy with a higher average value than the optimal strategy. The properties of the matrix make it possible to infer that the *Ss* who chose the optimal strategy repeatedly did so not because it dominated the other choices, nor because it had the highest average value, but because they were willing to forego the possibility of a larger win for the conservative value of the two-person, zero-sum game.

The results obtained appear to be a mixture of two types of behavior. One type, present in approximately half of the *Ss*, was conformity to the commands of the minimax model. Fifteen *Ss*, once they determined their opponent would be rational, after a number of plays of the game (ranging from 10 to 125), chose strategy 3, 100 per cent of the time. For these people, in the present situation the minimax solution appears to be an adequate explanation of their final level of response. After an a priori analysis of and some experience with the game, these individuals conformed to the mathematical equilibrium notion.

This still leaves half of the *Ss* whose behavior clearly deviated from the mathematical solution. However, it is worth noting that the large majority of the choices of this group are of the optimal strategy. And from Figure 1, it appears that after 200 trials the behavior may be still changing, approaching the optimal solution. An obvious question presents itself. Will those individuals who have not adopted the minimax strategy after 200 plays of the game eventually do so; or will they reach an asymptotic level of response below 100%?

Although there has been widespread interest in game theory for more than a decade, there is little empirical work that is directly related to this work. A study by Kaufman and Becker (1959) dealt with problems related to the current work. These authors had college students play five, two-person, zero-sum games; four requiring the players to utilize mixed strategies and a fifth having a saddle point. From their unpublished paper, it appears that their results differ somewhat from the current work. However, certain procedural differences prevent a direct comparison of the two pieces of work.

The results of this study shed some light on previous speculations which concluded that individuals do not behave as the game models prescribe. The current results enable us to say, tentatively, that in certain game situations, some intelligent individuals adopt minimax strategies and others do not. It is possible to speculate, and there is some evidence (Lieberman, 1959), that in situations simpler than the present one a larger proportion of people would be rational. In more complex situations (two-person games involving mixed strategies and *n*-person games), fewer people would be rational.

The situations of interpersonal conflict and interaction described by the game models appear to be both significant and general. Once the areas of failure of the normative model are discovered and confirmed, the construction of adequate descriptive theories of behavior in game situations would be of interest. The situations involved in two-person, zero-sum games are analogous to some of those described by the stochastic learning models. However, no adequate descriptive theory of behavior in matrix game situations exists.

In attempting to account for the behavior of the *Ss* who did not adopt the optimal strategy in the course of play, it is necessary to examine some of the assumptions underlying the study. People will repeatedly make rational choices only if they attempt to accumulate as much money as possible; and, given a rational opponent, are willing to accept the conservative, minimax value of the game. If *Ss* are indifferent to the loss of the small amounts of money involved, there is little to prompt them to behave rationally. In addition, it is necessary that the utility of the amounts of money involved be equivalent to (or at least linearly related to) their objective values.

It is possible that these assumptions were not completely satisfied in the current work. Some *Ss* stated that they did not care about

the small losses; they chose to behave nonrationally to "make the game more interesting." It is possible to think that for certain *S*s, in certain parts of the game, the values $-1¢$ and $-2¢$ were approximately equal in utility value to zero and the values $+1¢$, $+2¢$ and $+15¢$ had an inordinately high utility. Also, the certainty of the zero payoff may have reduced the utility of that outcome and the uncertainty involved in nonrational choices may have had an attractiveness for the *S*s.[5] These considerations are not meant to serve as an explanation for the nonrational behavior of the *S*s but they do serve to illuminate some of the considerations involved in such behavior.

REFERENCES

Atkinson, R. C., & Suppes, P. An analysis of two-person game situations in terms of statistical learning theory. *J. exp. Psychol.*, 1958, 55, 369–378.

Atkinson, R. C., & Suppes, P. Applications of a Markov model to two-person noncooperative games. In R. Bush & W. Estes (Eds.), *Studies in mathematical learning theory.* Stanford, Cal.: Stanford Univer. Press, 1959. Pp. 65–75.

Estes, W. K. Of models and men. *Amer. Psychologist*, 1957, 12, 609–617.

Flood, M. M. *Some experimental games.* Research Memorandum RM-789-1, The Rand Corporation, 1952.

Kaufman, H., & Becker, G. M. *An experimental comparison of empirical and game-theoretical strategies.* Research Report, Electric Boat Division, General Dynamics Corporation, 1959.

Lieberman, B. *Human behavior in a strictly determined 2 X 2 matrix game.* Research Memorandum, Department of Social Relations, Harvard University, 1959.

Royden, H. L., Suppes, P., & Walsh, K. *A model for the experimental measurement of the utility of gambling.* Technical Report No. 14, Contract Nonr 225 (17), Applied Mathematics and Statistics Laboratory, Stanford Univer., 1957.

Suppes, P., & Atkinson, R. C. *Markov learning models for multiperson situations, 1. The theory.* Technical Report No. 21, Contract Nonr 225 (17), Applied Mathematics and Statistics Laboratory, Stanford Univer., 1959.

Vinacke, W. E., & Arkoff, A. An experimental study of coalitions in the triad. *Amer. Sociol. Rev.*, 1957, 22, 406–414.

[5] For an experimental attack on this problem see: Royden, Suppes, and Walsh, 1957.

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Man is by nature a social animal, and an individual who is unsocial naturally and not accidentally is either beneath our notice or more than human. Society is something in nature that precedes the individual. Anyone who either cannot lead the common life or is so self-sufficient as not to need to, and therefore does not partake of society, is either a beast or he is a god.

ARISTOTLE, *Politics*

CRITIQUE AND COMMENT

Interest in the Whorfian hypothesis—that the characteristics of a language have determining influences on cognitive processes—is increasing, not only among linguists and social anthropologists, but also among general semanticists, psychologists, social psychologists, and sociologists. Here is an interesting attempt by a social psychologist to systematize some of the work in this problem area, by proposing a double dichotomy which gives rise to a discussion of the hypothesis at four levels.

A SYSTEMATIZATION OF THE WHORFIAN HYPOTHESIS

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INTRODUCTION

DURING the late 20's and throughout the 30's two American linguists—Edward Sapir and Benjamin Lee Whorf—strikingly formulated anew the view that the characteristics of language have determining influences on cognitive processes. Since the languages of mankind differ widely with respect to their structural, lexical, and other characteristics, it followed that monolingual individuals speaking widely different languages should, therefore, differ with respect to their symbolically mediated behaviors. This formulation immediately rekindled interest in this topic area among linguists and social anthropologists and, with the passage of time, their interest has been increasingly transmitted to general semanticists, psychologists, social psychologists, and sociologists. Currently, interest in the Whorfian hypothesis (at times also referred to as the Sapir-Whorf hypothesis, the Korzybski-Sapir-Whorf hypothesis, the linguistic Weltanschauung hypothesis, and the linguistic relativity hypothesis) is high—although there is no little difference of opinion as to its proper interpretation, its limits, its verifiability, and its validity. This essay represents an attempt by a social psychologist to systematize some of the work in this problem area.

SOME LANGUAGE-THOUGHT RELATIONSHIPS NOT SUBSUMABLE UNDER THE WHORFIAN HYPOTHESIS

Since psychologists have been crucially interested in the impact of verbal habits on other kinds of behavior from the very beginning of the emergence of psychology as a scientific discipline [viz., Ebbinghaus's work with nonsense syllables (1897) and Galton's with word association (1879)], it is probably desirable to begin our deliberations with a clear indication as to why most of these studies are *not* concerned with the phenomena of interest to students of the Whorfian hypothesis. The classical psychological interest in this area is in terms of language as a human capacity. Psychologists have not been noticeably concerned with the unique "Englishness" of English and how it affects cognition—in the individual and in the culture—differently than the "Navahoness" of Navaho. For the large number of American psychologists who have in recent years reported on the impact of verbal habits on perception (e.g., Solomon and Howes, 1951; Miller, Bruner and Postman, 1951; Postman and Conger, 1954; Postman and Rosenzweig, 1957), the fact that the language involved was English was of no particular concern. The same is true of those who have earlier and also recently studied the relationship between language

and learning (e.g., Lyon, 1914; Gates, 1917; Tsao, 1948), language and memory (e.g., Reed, 1924; Miller and Selfridge, 1950), and language and thinking (e.g., Eidens, 1929; Sells, 1936; Wilkins, 1928). Similarly, the studies of Piaget (1924), Vigotsky (1939), Luria (1959), and other European psychologists in this tradition have not been interested in the unique or characteristic verbal habits of *French* speaking or *Russian* speaking individuals and how such habits uniquely or characteristically affect *French* thinking or *Russian* thinking. Even those studies that have worked with various "levels of approximation" (in an information theory sense) to the actual word sequences or letter sequences of some language (usually American English) have not been reacting to anything different in this respect than they would have been interested in were they working with some entirely different written or spoken language.

Furthermore, the "other variable" in all of these studies (whether this other variable be perception, learning, memory, reasoning, etc.) is of interest to the psychologist only as it is manifested through verbal habits. Thus, Woodworth and Sells (1935) are not concerned with the impact of verbal or symbolic habits on non-verbal reasoning. Rather, they are interested in *reasoning with verbal or symbolic materials* to determine how such reasoning is affected by our long ingrained verbal or symbolic habits. Similarly, Postman and his students and colleagues (references cited above) have been concerned not with the impact of *verbal* habits on the perception (visual or auditory) of *non-verbal* stimuli, but, rather, with the extent to which and the ways in which verbal habits control the perception of verbal stimuli. In other words, most classical psychological research in this area is not really concerned with the relationship between verbal habits on the one hand and certain non-verbal or non-linguistic behaviors on the other.

Although the Whorfian hypothesis has been discussed via data at various levels of linguistic and non-linguistic behavior, it differs from most psychological research dealing with the impact of verbal habits on behavior either in *one* or in *both* of the respects indicated above:

a. Research pertaining to the Whorfian hypothesis is concerned with those verbal habits that derive not from the mere fact of having acquired language but, rather, with those verbal habits that derive from some characteristic or unique aspect of one or more *given* languages. Thus, the Whorfian hypothesis is not so much concerned with verbal determinism in general as it is with relative linguistic determinism based upon contrasts between the characteristics of specific languages.

b. Much, though by no means all, research on the Whorfian hypothesis is concerned with relating these data pertaining to the unique characteristic nature of one or more given languages to some non-linguistic behaviors of the individual speakers of these languages.

THE WHORFIAN HYPOTHESIS: A BRIEF HISTORICAL INTRODUCTION

As is the case with many of the most fertile ideas of interest to social scientists today, it is possible to trace back some of the ingredients which go to make up the Whorfian hypothesis at least to the mid-nineteenth century, if not further. That multifaceted genius, Wilhelm von Humboldt, ventured to say in 1848 that "man lives with the world about him principally, indeed . . . exclusively, as language presents it." (Cited by Trager, 1959.) Although we may not now agree that von Humboldt demonstrated this proposition, we must admit that he and his followers amassed a wealth of linguistic evidence from language groups all over the globe in the process of their dedication to this proposition. In fact, it is from their early efforts that ethnolinguistics arose as a recognizably separate area of scholarly activity. It was also in reaction to this activity that a particular brand of logical and scientific criticism developed which long steered the major body of American linguistics away from ethnolinguistic concerns. This criticism was derived from the superficiality of the data and the prematureness of the conclusions via which ethnolinguistics arrived at the conclusion

that the "soul of a people" and the "mind of a people" was not only reflected by its language but, indeed, shaped by it.

Humboldtian ethnolinguistics itself has long and intricate roots in the folklores of many peoples. The association between a people's individuality and its language is certainly an ancient one in the history of Western and Near Eastern civilizations. The ancient Greeks applied the term "barbarians" (those who say ba-ba, i.e. speak an incomprehensible tongue) to those to whom the gods had denied the gift of Greek, and the ancient Hebrews believed that their language was a uniquely holy vehicle that was created even before the world was brought into being and awarded to them as a particular gift of God. Certainly, linguistic awareness, linguistic pride, a belief in linguistic specialness and in the inherent untranslatability of one's own vernacular or some other superposed language have been frequent components of the ethnocentrism and the world-views of many peoples, past and present. The Whorfian hypothesis represents a groping toward the scientific restatement and the objective evaluation of language-and-behavior phenomena that are related to this ill-defined, pre-scientific, and value-laden area that has hitherto been shaped primarily by strong beliefs and emotions. The Whorfian hypothesis essentially represents an attempt to study linguistic relativity by the means of modern social science methods.

Among the earliest post-Humboldtian attempts to point out the linguistic relativity of symbolically mediated behavior were those made by the father of American anthropology, Franz Boas. An accomplished comparative linguist (i.e., a disciplined student of the structures of various languages), even by present-day standards, Boas claimed that a "purely linguistic inquiry" provided the data for "a thorough investigation of the psychology of the peoples of the world" (1911). The linguistic data obtained for this purpose Boas considered as superior even to that derivable from psychology directly, due to the fact that informants are more nearly unconscious of the categories in their thinking which language as such reveals.

With the writings of Edward Sapir, we approach the more direct and immediate antecedents of the Whorfian hypothesis. Sapir was a student of Boas and, like his teacher, firmly convinced that language could be regarded as the raw material of which a people's outlook on the world is fashioned. In 1929 Sapir wrote:

> ". . . The 'real world' is to a large extent unconsciously built up on the language habits of the group. The worlds in which different societies live are *distinct* worlds, not merely the same world with different labels attached. We [as individuals] see and hear and otherwise experience very largely as we do because the language habits of our community predispose certain choices of interpretation."

Here we find clearly expressed a theme to which Whorf and others have returned time and again, namely that language is not "merely" a vehicle of communication by which man talks about some objective reality "out there" that exists previous to and independently of his language, but, rather, that language itself represents an objective reality by means of which man structures and organizes the "out there" in certain characteristic ways. Thus, when languages differ maximally, the organizing schemata which their speakers impose on the non-linguistic world should also differ maximally. In his own words, Sapir claims that language "does not as a matter of actual behavior stand apart from or run parallel to direct experience—but completely penetrates with it" (1933).

Although Sapir was certainly interested in linguistic relativity and convinced of its validity, his primary scientific and speculative contributions were made in connection with other topic areas. Benjamin Lee Whorf, on the other hand, devoted himself to the advancement of the hypothesis that now carries his name almost from the very beginning of his systematic work in linguistics up until the time of his premature death. In a series of articles—most of which have at long last been gathered together in one publication (Carroll, 1956)—based primarily on analyses of American Indian

languages (but also, at times, based on other non-European languages), Whorf constantly reiterated his conviction that a generally unrecognized "principle of relativity" was operative. In accord with this principle, "observers are not led by the same picture of the universe, unless their linguistic backgrounds are similar or can in some way be calibrated" (1940).

Whorf's writings have become a common ground on which the interests of many linguists, anthropologists, social psychologists, and sociologists come together. Its attraction lies both in its dimly recognized reference to the classical problems of cultural differences, cultural relativity, and cultural universality as well as in its provocativeness for modern, interdisciplinary, objective research. When Whorf says that "there is a precarious dependence of all we know upon linguistic tools which themselves are largely unknown or unnoticed," he hits all of us where it hurts most—at the foundations of our certainty in our scientific findings and in our everyday decisions. When he attacks the view that grammars are "merely norms of conventional and social correctness" and claims that they are, instead, the cement out of which we fashion experience, we feel that he must either be pointing at an unnoticed and potentially dangerous popular fallacy or tilting at nonexistent windmills. When he says that "we cut up nature—organize it into concepts—and ascribe significances as we do . . . largely because of the . . . absolutely obligatory . . . patterns of our [own] language," he stirs in us both our ethnocentric group-pride as well as our universalistic anti-ethnocentrism. In short, Whorf (like Freud) impugns our objectivity and rationality. It is not surprising then that recent years have seen many logical as well as not a few experimental efforts to evaluate and re-evaluate both the conceptual and the empirical grounds upon which the Whorfian hypothesis rests.

LEVEL 1. LINGUISTIC CODIFIABILITY AND CULTURAL REFLECTIONS (THE FIRST LANGUAGE-LANGUAGE LEVEL)

The weakest level of the Whorfian hypothesis (in the sense of being least pretentious or least novel) is that which provides evidence that languages differ "in the same ways" as the general cultures or surrounding environments of their speakers differ. Evidence along these lines has long been provided by ethnologists and folklorists, and its fragmentary and belated presentation by Whorfians can hardly be considered as either a serious contribution to the social sciences generally or as a substantiation of higher levels of the Whorfian hypothesis specifically.

From the point of view of the language data presented at this first level of argumentation, it is not the grammatical structure as such that is under consideration but, rather, the lexical store or the so-called "semantic structure." Actually, that which is dealt with at this level might be referred to in present-day terms as contrasts in *codifiability*. Language X has a single term for phenomenon x, whereas language Y either has no term at all (and therefore refers to the phenomenon under consideration—if at all—only via a relative circumlocution) or it has three terms, y_1, y_2, and y_3, all within the same area of reference. As a result, it is much *easier* to refer to certain phenomena or to certain nuances of meaning in certain languages than in others. Thus, codifiability is also related to the question of translatability and to "what gets lost" in translation from one language to another.

The examples that have been given at this level of analysis are legion and no attempt needs to be made to exhaustively catalog them here. At times they are not accompanied by any explicit claims as to how the noted codifiability differences are related to implicit or explicit cultural differences. The fact that there is no handy English equivalent for the German *Gemütlichkeit* is rarely explained as an indication that Germans *are* more *gemütlich*. The fact that there may not be an exact semantic ("connotative") equivalence between *horse*, *cheval*, and *pferd* is not seriously attributed to different cultural roles for horses or for man-horse relationships in the three cultures from which these words are derived.

Nevertheless, there have been attempts to relate codifiability differences of the above type to behavioral differences. Usually, the

behavior under scrutiny in this connection is *language behavior*, and the evidence advanced as to the parallelism between *selective codifiability as an aspect of a given language* and *the language behavior of the speakers of that language* is, at this level of analysis, of an anecdotal or fragmentary variety. Thus, the fact that the German language does have the term *Gemütlichkeit* does make it *easier* for Germans to be aware of and to express this phenomenon. Americans can also struggle toward a circumlocutious formulation of this concept but the very fact that it is a struggle may mean that the concept is less clearly formulated and less aptly as well as less frequently expressed. Gastil's concept of *polysemy* (1959) is also relevant at this level. In some languages, certain words have additional shades or ranges of meaning than their cognates or best equivalents in other languages. Thus, in French, one term is used for both "conscience" and for "consciousness." On the one hand, this means that French speakers do not have as *easily* available to them a distinction that we have. On the other hand, it means that they have more easily available to them a partial identity of these two terms that it is very difficult for us to fully appreciate. Lindeman (1938) has demonstrated from textual analysis how this linguistic identity has led to a greater conceptual fusion between these two usages on the part of French philosophers than is true for English or German thinkers. Gastil gives several other examples of polysemy (1959), particularly as between English and Persian, which convincingly demonstrate that code efficiency is differentially selective in different languages with the result that what is easily expressible in one language is not necessarily easily or accurately expressible in another.[1]

Assuming that the above holds true, the question still remains as to whether these observations are really pertinent to the Whorfian hypothesis. Seemingly so, for Whorf himself frequently presents data of this variety. Admittedly Whorf's examples are largely drawn from American Indian languages (and contrasted with American English), and the implication is therefore strong that we are not only dealing with groups whose languages differ markedly but whose lives and outlooks also differ greatly. Nevertheless, at *this* level of analysis, Whorf (and others even more frequently than he) does not take pains to relate linguistic factors to non-linguistic ones, but merely presents an enchanting catalog of codifiability differences. English has separate words for "pilot," "fly (n.)," and "airplane," but Hopi has only one. Eskimo has many words for different kinds of "snow" but English has only two. On the other hand, Aztec has only one basic word for our separate words "cold," "ice," and "snow." We have one word for "water," whereas Hopi has two, depending on whether the water is stationary or in motion. English has such words as "speed" and "rapid," whereas Hopi has no real equivalents for them and normally renders them by "very" or "intense" plus a verb of motion. English has separate terms for "blue" and "green" but only one term for all intensities of "black" short of "gray." Navaho, on the contrary, does not have separate highly codeable terms for "blue" and "green" but does have two terms for different kinds of "black." English has the generic term "horse" but Arabic has only scores of different terms for different breeds or conditions of horses. The kinship terminology in some languages is certainly vastly different (and in certain respects both more refined and more gross) than it is in English. In all of these cases, it is not difficult to relate the codifiability differences to gross cultural differences. Obviously, Eskimos are more interested in snow, and Arabs in horses, than are most English speakers. Obvious, also, is the fact that these codifiability differences help speakers of certain languages to be more easily aware of certain aspects of their environment and to communicate more easily about them. This, essentially, was the lesson

[1] I have previously pointed to codifiability as a precursor of language behavior in such diverse areas as social sterotyping (Fishman, 1956) and witness performance (Fishman, 1957). In general, to the degree that we are here dealing with codifiability in the absence of any consistent cultural context, our concern with the impact of language habits on language performance is only a shade different than that of the studies that we have previously characterized as not being subsumable under the Whorfian hypothesis.

we learned from Bartlett's early work on remembering (1932). In this sense, then, their languages structure their verbal behavior in a non-trivial way and ostensibly also structure their pre-verbal conceptualizations as well.

Other phenomena

Before proceeding to a summary and evaluation of this particular level of the Whorfian hypothesis, it might be well briefly to point out other phenomena that are subsumable under it. The "phatic communion" concept which Malinowski introduced so long ago (1923), and which so many have quoted but which few have elaborated, may in large part be seen as a codifiability phenomenon at the language-language level. It is probable that all traditioned groups, particularly those sharing many intimate and heightened experiences, develop differential codifiability in their languages. As a result, it is quite likely that they then find it easier, more meaningful, and more accurate to communicate their unique or characteristic experiences in their own language than in any other. The elliptic, highly abbreviated, seemingly mysterious, and non-verbal aspects of "phatic communion" (to outsiders) may be no more than a result of the differences between what are readily encodeable messages (verbal and non-verbal) for any group in its own language, and what are readily encodeable (and therefore decodeable) messages for some other group of observers who are "outsiders." In a sense, we are dealing once more with the relative translatability of the supposedly untranslatable. Since differential codifiability is so very often a correlate of distinctive cultural patterns, it seems quite understandable why most speakers of a language come to feel that no other language can as adequately cope with the very nuances of meaning and the very experiential patterns which are most significant to them in terms of their cultural distinctiveness. From this awareness it is but a short jump to language loyalty and language glorification. Thus, "word magic" may not be nearly as primitive, childish, or illogical as Freud suggests. There is magic indeed in a language whose differential codifiability makes it peculiarly suitable for the expression of an individual's most central personal and cultural experiences. In contrast to such a language, all others must seem pale indeed.

In conclusion, it must be said that the evidence for the Whorfian hypothesis at this first level is concerned neither with a truly *structural* analysis of language nor with a full-blooded analysis of the *non-linguistic concomitants* or resultants of language structure. In dealing with language-language relationships, the data are presented in a seemingly anecdotal and selective manner which fails to convince those who require more disciplined and organized approaches both to data and to demonstration. Nothing as grandiose as a "world-view" is produced by data at this level, although quite frequently we must admit that the reporter gives much evidence of having fully immersed himself in the language and culture under study. Whorf himself is truly amazing in this last respect. Nevertheless, this first level is certainly a comedown from the more advanced levels of argumentation that he pursued. Perhaps this can be explained by his sheer fascination—often quite emotional in tone—with the American Indian languages which he had mastered. Undoubtedly the great structural differences between these languages and those that he almost disdainfully called "Standard Average European" (1941) provided much of the intellectual stimulation for his linguistic relativity hypothesis. On the other hand, he seems so fond of "playing" with these languages, fondling and dissecting their units of expression, that he may have been unable to withstand the temptation to do so even when he was not contributing thereby to the ultimate argument that he sought to advance. If we are to be satisfied only with the first level just discussed, then we can conclude no more than has Gastil in his discussion of polysemy, namely that

> "Languages differ as to the presence or absence of the field distinctions which they make. A language may be seen as a limited group of words and forms available for the use of a man thinking or expressing himself in the medium of that language. If he does not have the means to do a certain job of thinking or expressing, that job will

not be accomplished as well as if he had such means." (1959)

Whorf would probably not have settled for as limited a claim as the foregoing.

LEVEL 2. LINGUISTIC CODIFIABILITY AND BEHAVIORAL CONCOMITANTS

At the second level of analysis of the Whorfian hypothesis, we leave behind the limitations of *inference* from codifiability in language to ease of formulation or expression via language. That is to say, we leave behind the *language-language behavior* level for the level in which *language-nonlanguage behavior* becomes of paramount interest to us. That this is a necessary direction for our inquiry to take has been recognized by Carroll and Casagrande who write in a recent preview of a forthcoming book:

"In order to find evidence to support the linguistic relativity hypothesis it is not sufficient merely to point to differences between languages and to assume that users of these languages have correspondingly different mental experiences. If we are not to be guilty of circular inference, it is necessary to show some correspondence between the presence or absence of a certain linguistic phenomenon and the presence or absence of a certain kind of non-linguistic response." (1958)

Note that the above quotation merely refers to "*a certain linguistic phenomenon*" rather than restricting the *type* of linguistic phenomenon that requires attention. The hallmark of the second level is that the "predictor" variables seem once more to be of the lexical or semantic codifiability type (and in this respect similar to Level 1, discussed above), whereas the "criterion variables" are of the non-linguistic behavior type (and in this respect different from, and an advance over, those encountered at Level 1). Thus far, there have been only a very few studies which strike me as operating at this level of analysis. The earliest one by far is that of Lehmann (1889) who demonstrated that identifying a different number with each of nine different shades of gray was of substantial help in behaviorally discriminating between these shades of gray. In essence, then, the numbers functioned as verbal labels. The availability (codifiability) of such labels for some *S*s resulted in much better discrimination-identification of the shades of gray than that which obtained in other *S*s who had to perform the same discrimination-identification task without being provided with such labels.

Some exceptionally interesting and sophisticated work with the codifiability concept in the color area has more recently been reported by Brown and Lenneberg (1954) and by Lenneberg alone (1953, 1957). These investigators have shown that culturally encoded colors (i.e., colors that can be named with a single word) require a shorter response latency when they need to be named than do colors that are not culturally encoded (i.e., that require a phrase—often an individually formulated phrase—in order to be described). At this point, their evidence pertains to Level 1 that we have previously discussed. In addition, these investigators have gone on to show that the more highly codified colors are more readily recognized or remembered when they must be selected from among many colors after a period of delay subsequent to their original presentation. This finding was replicated among speakers of English and speakers of Zuni, although somewhat different segments of the color spectrum were highly codeable for the two groups of *S*s. The investigators summarize their findings to this point as follows:

"It is suggested that there may be general laws relating codability to cognitive processes. All cultures could conform to these laws although they differ among themselves in the values the variables assume in particular regions of experience." (Brown and Lenneberg, 1954)

Going on from this original statement, Lenneberg (1957) has further refined its experimental underpinnings by showing that the *learning* of color-nonsense syllable associations was predictably easier or harder as the learning task involved color categories that varied in degree from the ones that were most commonly recognized by his English-speaking *S*s. He therefore concluded that "there is good evidence that the shape of word frequency distributions over stimulus continua regulates the ease with which a person learns to use a word correctly." This conclusion should be as applicable to original language learning as it is to

second and to artificial language learning, for it basically pertains not to language usage per se but to concept formation as such.

The color continuum seems to be a particularly fortunate area in which to study codifiability-cognition phenomena precisely because it is a real continuum. As such, no "objective" breaks occur in it and it is a matter of cultural or sub-cultural consensus as to just which breaks are recognized, just where on the spectrum they are located, and how much of a range they include. The demonstration that these various codifiability considerations influence recognition, recall, and learning has been most fortunately executed. Lenneberg and Brown are also alert to the fact that at this level it is perfectly acceptable to work with intralinguistic designs rather than to necessarily utilize the interlinguistic designs in terms of which the Whorfian hypothesis is most frequently approached. What is easily codifiable, and the specific range and content of easily codeable categories, does depend on the particular language under consideration. It also depends on the particular experiences of subgroups of speakers. As a result, contrasts in rate, ease or accuracy of various cognitive functions should be (and are) demonstrable both intralinguistically and interlinguistically as a function of codeability norms. Intralinguistic codifiability-cognition differentials in various natural population groupings should be of particular interest to students of social stratification.

Brown and Lenneberg have conducted their work with a conscious awareness of the Whorfian hypothesis and how it must be further specified or delimited. On the other hand, there have been other investigators who have also worked in the language-behavior domain at this level without any particular awareness of the Whorfian hypothesis as such. If the organizational framework here being advanced has been insightfully developed, it should nevertheless be possible to subsume their findings within it. In fact, it may turn out that within the context of the Whorfian hypothesis these other studies will obtain a new coherence and provocativeness. As a start in this direction (and an exhaustive search of the literature would be necessary in order to seriously go beyond such a start), I would classify the oft-cited work of Carmichael, Hogan, and Walter (1932) on memory for visual stimuli to which different verbal labels have been attached, as well as the problem-solving work of Maier (1930), and the transactional studies summarized by Kilpatrick (1955) on perceptual learning and problem-solving with and without verbal set as belonging at this level of analysis. These three last-mentioned studies also utilize intralinguistic designs.

The only study at this level that is directly inspired by the Whorfian hypothesis while utilizing an *interlinguistic* design is the one which Carroll and Casagrande refer to as "Experiment I" (1958). In this study, fluent Hopi speakers were compared with two different groups of English speakers with respect to sorting or picture classifying behavior. Utilizing the kind of lexic data that we have discussed in connection with Level 1, Carroll hypothesized that in each set of three stimulus plates Hopi speakers would more usually classify a certain subset of two as belonging together (because in Hopi the seemingly dissimilar activities depicted were nevertheless commonly "covered" by a single verb) whereas English speakers would more frequently classify another subset of two as belonging together (because in English a single verb was commonly used in referring to them). Although Carroll's data are quite far from revealing a uniform association between "Hopi categorizing responses" and Hopi speakers, or between "English categorizing responses" and English speakers, they nevertheless do reveal quite a substantial tendency in that direction. In addition, his graduate student *Ss*, whom we might have expected to be more "verbally minded," give the predicted "English categorizing responses" much more frequently than do his rural white *Ss*.

All in all, this is certainly an experimentally exciting level of analysis and one which will undoubtedly develop further in the years ahead. It will certainly be subject to improved experimental techniques since those now working in it are themselves

aware of improvements that they hope to introduce into their future work in this area. This would seem to be the level of the Whorfian hypothesis most likely to attract social psychologists and sociologists with empirical interests in language phenomena.

LEVEL 3. LINGUISTIC STRUCTURE AND ITS CULTURAL CONCOMITANTS

When we turn our attention from the second to the third and fourth levels of the Whorfian hypothesis, we progress from lexical differences and so-called "semantic structure" to the more "formal" and systematized grammatical differences to which linguists have most usually pointed when considering the structure of a language or structural differences between languages. There is some evidence that although Whorf and others may, at times, have reverted to lower levels of presentation and documentation they, nevertheless, did associate linguistic relativity in its most pervasive sense with structural (i.e., grammatical) rather than merely with lexical aspects of language. This is suggested by such formulations as Sapir's that meanings are "not so much discovered in experience as imposed upon it, because of the tyrannical hold that linguistic *form* has upon our orientation to the world" (1919, my italics). Somewhat more forcefully stated is Whorf's claim that "the world is presented in a kaleidoscopic flux of impressions which has to be organized . . . largely by the linguistic *systems* in our minds" (1940, my italics). More forceful still—and there are a large number of possible quotations of this kind—is Whorf's statement that

> ". . . the background linguistic system (in other words, the grammar) of each language is not merely a reproducing instrument for voicing ideas, but rather is itself the shaper of ideas, the program and guide for the individual's mental activity, for his analysis of impressions, for his synthesis of his mental stock in trade. Formulation of ideas is not an independent process, strictly rational in the old sense, but it is part of a particular grammar and differs, from slightly to greatly, between grammars." (1940)

Finally, we may offer in evidence the paraphrasings of the Whorfian hypothesis by two eminent American linguists who have been both interested in and sympathetic to this hypothesis. The first of these says simply that "It is in the attempt properly to interpret the *grammatical categories* of Hopi that Whorf best illustrates his principle of linguistic relativity" (Hoijer, 1954, my italics). The other, as part of a more extended and systematic argument, says

> "Language as a whole has structure and all of its parts and subdivisions also have structure . . . [if] the rest of cultural behavior has been conditioned by language, then there must be a relationship between the *structure* of language and the *structure* of behavior." (Trager, 1959)

The emphasis on language *structure* as the critical feature in his linguistic relativity hypothesis is actually a later and more mature level of Whorf's own thinking in this area. Sapir, on the other hand, as a professionally trained and professionally oriented linguist, was quite probably interested in language structure from the very outset. Whorf's intellectual and technical development in this topic area shows a transition from diffuse and unsystematic lexical analyses to more focused and interrelated grammatical analyses, the turning point coming most noticeably after his studies with Sapir. Be this as it may, both levels of argumentation do appear in Whorf's writings and even his later writings reveal many instances of regression to the first level discussed above. Thus, it is not strange that in the deliberations of many other students of this problem, whether in the roles of protagonists or detractors, the distinction between the lexical and the grammatical levels of analysis has not been fully exploited.

At the third level of analysis, we once more find ourselves in a realm of rich though ambiguous anthropological and ethnological data. As was the case with Level 1, above, the direct association or chain of reasoning between grammatical structure on the one hand and "something else" (be it *Weltanschauung* or even some less embracing segment of culture or values) on the other is not explicitly stated. Often, the "something else" is not stated at all and yet there is the general implication that grammatical oddities of the type presented cannot help but

be paralleled by unique ways of looking at or thinking about or reacting to the surrounding environment. Thus, one encounters such evidence as that Chinese has no singular and plural or that it has no relative clauses (which we English speakers *do* have), whereas other languages have more levels of grammatical number (including singular, dual, tri-al, and plural forms—which we English speakers do *not* have). In this vein, the cataloging of grammatical differences can continue at great length (languages that do recognize gender of nouns and those that do not, languages that have tenses and those that do not, etc.); for both anthropologists, linguists, and a variety of non-specialists have contributed to the fund of knowledge of phenomena of this type, always with the implication that it is clearly illogical to seriously suggest that linguistic phenomena such as these would have no relationship to life, to thought, and to values.

On the other hand, there are also several investigators that *have* attempted to indicate what the "something else" might be. In contrasting Hopi with English, Whorf (1940) has pointed to such odd grammatical features in Hopi as the absence of tenses, the classification of events by duration categories such that "events of necessarily brief duration (lightning, wave, flame, meteor, puff of smoke, pulsation) cannot be anything but verbs," the presence of grammatical forms for indicating the type of validity the speaker intends to attribute to his utterance (statement of current fact, statement of fact from memory, statement of expectation, and statement of generalization or law), etc. To Whorf all of these grammatical features seemed congruent with an outlook on life that was "timeless" and ahistorical in the sense that past, present, and future are seen as a continuity of duration, experience being cumulative and unchanging for countless generations. As a result of the "timelessness" of Hopi life, it is of greater importance for Hopi speakers to distinguish between the duration of events and their certainty than to indicate when they occurred (1941). A similarly ingenious and sensitive analysis is followed by Hoijer (1951, 1954) in connection with the Navaho verb system in which there is no clean separation between actors, their actions, and the objects of these actions. As Hoijer sees it, the Navaho verb links the actor to actions which are defined as pertaining to classes-of-beings. Thus it would appear that people merely "participate in" or "get involved in" somehow pre-existing classes of actions rather than serve as the initiators of actions. Hoijer interprets these grammatical characteristics as being consistent with the "passivity" and "fatefulness" of Navaho life and mythology in which individuals adjust to a universe that is given. Finally, in Nootka, Whorf finds a connection between the absence of noun-verb distinctions and "a monistic view of nature" (1940).

An example of work of this kind with modern European languages is provided by Glenn (1959). He points to the fact that the adjective most commonly precedes the noun in English whereas it most commonly follows it in French. The former pattern Glenn considers to be descriptive in a narrow or particularistic sense, more in keeping with inductive thought. The latter pattern he considers to be classificatory in a manner that moves from broader to narrower categories, as is the case with deductive thought. Glenn then proceeds to find "inductiveness" in English behaviors of various kinds (an inductive legal system in which many minute precedents culminate in the inductive formation of the common law, local governmental jurisdictions, and greater attention to pragmatic detail rather than to all-embracing theoretical unities, etc.) and "deductiveness" in diverse areas of French behavior (a broad legal code first and then individual decisions deduced from it, centralized governmental authority, the primacy of broad theoretical and philosophical interests, etc.). Whorf, too, has dealt with the world view of Standard Average European speakers, concluding from grammatical analyses that it emphasizes the concrete or material and the quantifiable (1941).

The efforts by Whorf, Hoijer, Glenn and similar scholars (see, e.g., Boas, 1938; Lee, 1944) merit considerable respect. They must be separated in our evaluation from pseudo-serious efforts to attribute or relate the

musicalness of Italians to the light, melodious nature of the Italian language, or the stodginess of Germans to the heavy, lugubrious quality of the German language, or the warm, folksiness of Eastern European Jews to the intimate emotional quality of Yiddish, etc.[2] Superficially, the two approaches may seem similar, but the latter approach does not even have a serious structural analysis of language to recommend it. Nevertheless, the appeal of the Whorfian hypothesis for some lies precisely in the fact that it attempts to apply modern scientific methods and disciplined thought to such "old chestnuts" as the presumed "naturalness" that Hebrew (or Greek, or Latin, or Old Church Slavonic) be the language of the Bible, given its "classic ring" and its "otherworldly purity." However, with all of our admiration for those who have had the temerity as well as the ingenuity to attempt a rigorous analysis at this level, we must also recognize the limitations which are built into this approach. As many critics have pointed out (see, e.g., Lenneberg, 1953; Gastil, 1959), the third level of analysis has not normally sought or supplied independent confirmation of the existence of the "something else" which their grammatical data is taken to indicate. As a result, the very same grammatical designata that are said to have brought about (or merely to reflect) a given *Weltanschauung* are also most frequently the only data advanced to prove that such a *Weltanschauung* does indeed exist. Thus, once more, we are back at a language-language level of analysis (language structure ↔ language-behavior-as-indication-of-world-view). Perhaps social scientists working together with linguists (or individuals trained in both fields) will ultimately be able to overcome this limitation as other evidence concerning national character and cultural mainsprings is amassed and systematized. As soon as there is sufficient "other" data (based on analysis of folklore materials, personality data, value-orientation measurements, etc.) and sufficient agreement as to how such data should be interpreted, there might well be a kind of Human Relations Area Files from which systematic variations in grammatical structure and systematic variations in value structure or life-style might be determined. This may not be too different from what Trager (1959) has had in mind in calling for greater attention to the recognition of structure in non-verbal areas of culture in order to push forward with research on the Whorfian hypothesis.

Pending the availability of such data, we might well consider that it is not the language-language nature of argumentation at this level that is, per se, its greatest drawback but rather its susceptibility to selective presentation and to biased interpretation. Verbal behavior may long continue as our major avenue of insight into values and motives. What we must be ever more dissatisfied with, however, are the self-selected lists of grammatical examples and the self-selected enumerations of cultures, cultural values or themes, and the evidence pertaining to the existence of such themes. In attempting to avoid these particular pitfalls, students of the Whorfian hypothesis have increasingly come to express a preference for a study design which investigates the relationship between grammatic structure on the one hand and *individual* non-linguistic behavior on the other. Although this is both a logical and a very promising solution to many of the above-mentioned problems, there is nevertheless no need to conclude at this point in our knowledge that it is the only one possible.

LEVEL 4. LINGUISTIC STRUCTURE AND ITS BEHAVIORAL CONCOMITANTS

The conceptual and methodological superiority of the fourth level of the Whorfian hypothesis is one thing. The accessibility of this level for study may well be quite another thing. It does seem that this level is in some ways the most demanding of all, for it requires detailed technical training at both the predictor and the criterion ends of the relationship to be investigated. This may be the reason why there currently appears to be only one study which might possibly be

[2] Whorf himself makes a disclaimer along these lines when he states "I should be the last to pretend that there is anything so definite as 'a correlation' between culture and language, and especially between enthnological rubrics such as 'agricultural, hunting,' etc. and linguistic ones like 'inflected,' 'synthetic,' or 'isolating.' " (1941)

said to be an example of work at this level,[3] although in the future we might expect it to elicit greatly increased interest among sociolinguists and social psychologists with technical linguistic training. This is the study by Carroll and Casagrande which they refer to as Experiment II (1958). The grammatic features of interest to Carroll and Casagrande in this study are the particular verb forms required in Navaho verbs for handling materials in accord with the shape or other physical attribute (flexibility, flatness, etc.) of the object being handled. Note that Carroll and Casagrande are concerned here with distinctions in verb *forms* rather than distinctions between mere lexical absence or presence of verbs as such. Presumably it is this fact which permits us to consider Experiment II as a Level 4 study rather than as a Level 2 study. The nonlinguistic data utilized by Carroll and Casagrande are the object-classifying behaviors of their *S*s when presented first with a pair of objects which differ from each other in *two* respects (e.g., color and shape) and then with a third object similar to each member of the original pair in one of the two relevant characteristics. The *S*s were asked to indicate "which member of the (original) pair went best with the (third) object shown him." If the *S*'s reaction was governed by the requirements of Navaho verbal form, he would have to select a certain one of the original set of objects.

Carroll and Casagrande's hypotheses are quite explicitly stated: "(a) . . . that this feature of the Navaho language would affect the relative potency or order of emergence of such concepts as color, size, shape or form, and number in the Navaho-speaking child (specifically, that shape or form would develop earlier and increase more regularly with age, since this is the aspect provided for in the verb forms themselves), and (b) that he (i.e., the Navaho child) would be more inclined to perceive formal similarities (i.e., shape or form similarities) between objects than would English-speaking Navaho children of the same age." Carroll and Casagrande also assure us that the verb stems in question

> "compromise what Whorf has called *a covert class* and in the absence of native grammarians the pertinent grammatical rules operate well below the level of conscious awareness. Although most Navaho-speaking children, even at age 3 or 4, used these forms unerringly, they were unable to tell *why* they used a particular form with any particular object. Even when a child could not name an object—or may not have seen one like it before—in most cases he used the right verb form according to the nature of the object."

This last concern points up the importance of using unsophisticated *S*s in this area of research in the same way as this is an important requirement in many attitudinal, motivational, or other dynamic fields of inquiry.

Carroll and Casagrande's original *S*s were two very different groups of Navaho children—one being described as "Navaho dominant" (i.e., *S*s who were either monolingual speakers of Navaho or else were bilinguals in whom Navaho speaking was dominant over speaking English) and the other as "English dominant." Finally, a further control group was obtained consisting of white middle class children in the Boston area. All subjects were of roughly comparable ages. In many respects Carroll and Casagrande's findings are extremely favorable for the Whorfian hypothesis. The Navaho-dominant Navaho *S*s make the choices predicted by Navaho verb-stem requirements significantly more frequently than do the English-dominant Navaho *S*s. In addition, there is quite a consistent increase in the "Navaho required responses" with age, although the Navaho-dominant *S*s make such responses more frequently at every age from 3 through 10. Thus, when only these two groups of *S*s are considered, the evidence is quite favorable to the Whorfian hypothesis—even though there is far from a one-to-one relationship between language dominance and object-classifying behavior. However, when the data from the Bostonian *S*s are considered, some provocative difficulties appear. The white middle class children from the Boston area are even more Navaho in their object-classifying

[3] Related to this level of demonstration, but dealing with artistic structure rather than linguistic structure, are the findings summarized by Hallowell (1951) on the impact of culturally defined artistic styles on individual perceptions of reality.

behavior than the Navaho-dominant *Ss*![4] This superficially embarrassing finding is nevertheless of great provocative value both to Carroll and Casagrande and to all of those with concerns in this area, for it forces a consideration not only of the absence or presence of linguistic relativity in cognitive processes but of the degree ("strength") of this relativity as well as of its relative strength in comparison to other factors that may affect the direction of cognitive processes in various human groups. Perhaps, then, this may be the appropriate place to pause to consider this very matter.

THE DEGREE OF LINGUISTIC RELATIVITY

The fascination of the Whorfian hypothesis is in some ways compounded of both delights and horrors. We have already speculated concerning the delights. Let us now mention the horrors. The first is the *horror of helplessness*, since all of us in most walks of life and most of us in all walks of life are helplessly trapped by the language we speak. We cannot escape from it—and, even if we could flee, where would we turn but to some other language with its own blinders and its own vice-like embrace on what we think, what we perceive, and what we say. The second horror is the *horror of hopelessness*—for what hope can there be for mankind?; what hope that one group will ever understand the other?; what hope that one nation will ever fully communicate with the other? This is not the place for a full-dressed philosophical attack on these issues. Let us merely consider them from the point of view of the kinds of evidence supplied by some of the very studies we have mentioned.

The most "reassuring" facts that derive from Levels 1 and 2, the lexical and semantic codifiability levels of the Whorfian hypothesis, are that the noted non-translatability and the selective codifiability really pertain not so much to all-or-none differences between languages as to differences in relative ease or felicity of equivalent designation. Whenever we argue that there is no English word (or expression) for ——, which means so-and-so (or approximately so-and-so, or a combination of Y and Z) in English, we are partially undercutting our own argument. In the very formulation of our argument that there is "no English word (or expression) for ——" we have gone on to give an English approximation to it. This approximation may not be a very successful one but if that becomes our concern we can go through the contortions (both intellectual and gesticulational) that are required for an inching up on or a zeroing in on the non-English word or expression that we have in mind. The amount of effort involved may, at times, be quite considerable and even the final approximation may leave us dissatisfied. However, after all is said and done, this is not so different, in terms of both process and outcome, as the communication problems that we face with one another even within our *own* speech community. We can do no better than to quote Hockett's conclusions at this point, in support of what has just been said.

> "Languages differ not so much as to what *can* be said in them, but rather as to what it is *relatively easy* to say in them. The history of Western logic and science constitutes not so much the story of scholars hemmed in and misled by the nature of their specific languages, as the story of a long and fairly successful struggle *against* inherited linguistic limitations. Where everyday language would not serve, special sub-systems (mathematics, e.g.) were devised. However, even Aristotle's development of syllogistic notation carries within itself aspects of Greek language structure.
>
> "The impact of inherited linguistic pattern on activities is, in general, *least* important in the most practical contexts and most important in such "purely verbal" goings-on as story-telling, religion, and philosophizing. As a result, some types of literature are extremely difficult to translate accurately, let alone appealingly." (1954)

Turning now to Levels 3 and 4, where we become concerned with the imbedded

[4] Note: "I have just within the past couple of months gotten additional data from an age and sex matched group of Harlem school children. Though by no means affording a perfect control group, I think it is a closer match to my English dominant Navahos than educationally advanced middle class youngsters from Boston. . . . On inspection the results show the Harlem group to be very close to the English dominant Navahos and, of course, the same age trend shows up." (Casagrande, 1960)

structural features of a language, it seems to be important that we realize that Whorf never proposed that *all* aspects of grammatical structure must *inevitably* have direct cognitive effects. Thus, to begin with, we are faced with the task of locating those few grammatical features which might have definable but unconscious functional correlates in our unguarded behavior. This is what Hoijer has in mind when he points to

> "Mark Twain's amusing translation of a German folktale into English, where he regularly translates the gender of German nouns by the English forms *he*, *she*, and *it*. [This] illustrates in caricature the pitfalls of labeling the purely grammatical categories of one language in terms of the active structural-semantic pattern in another." (1954)

Lenneberg too has pointed to this very same principle (1953) when cautioning us that differences in metaphorical development in various languages may not only have no differential conscious correlates, but no differential unconscious ones as well.

If we look to Levels 2 and 4, these being the levels in which the behavioral concomitants of linguistic features are experimentally derived, we once more must reach the conclusion that linguistic relativity, where it does exist, is not necessarily an awesomely powerful factor in cognitive functioning. The relationships that have been encountered, though clear-cut enough, seem to be neither very great nor irreversible in magnitude. The very fact that increased infant and early childhood experience with toys and objects requiring primarily a form reaction can result in a *Navaho-like classifying preference* among monolingual English-speaking children also means that other kinds of environmental experiences might very well produce an *English-like classifying preference* among monolingual Navaho-speaking children. No one has yet directly studied the success with which behaviors predicted on the basis of linguistic relativity can be counteracted by either (a) simply making *S*s aware of how their language biases affect their thinking or (b) actively training *S*s to counteract these biases. It may be, after all, that this is an area in which *S*s can, with relatively little effort, learn how to "fake good." Furthermore, one might suspect that the impact of language *per se* on cognition and expression ought somehow to be greater and more fundamental than the impact of one or another language feature. Thus the impact of language *determinism* upon cognition ought to be more pervasive and more difficult to counteract than that of language *relativity*.

None of the foregoing should be interpreted as implying that linguistic relativity, wherever it exists, is an unimportant factor in human experience or one that deserves no particular attention except from those who are professionally committed to unravelling the unimportant in painful detail. Quite the contrary; just because it is such a seemingly innocuous factor it is very likely to go unnoticed and, therefore, requires our particular attention in order that we may appropriately provide for it.

SUMMARY AND CONCLUSIONS

The four levels of the Whorfian hypothesis that have been presented here are essentially subsumable under a double dichotomy. As Figure 1 reveals, we have essentially been dealing with two factors—one pertaining to

Data of Language Characteristics	*Data of (Cognitive) Behavior*	
	Language data ("cultural themes")	Non-linguistic data
Lexical or "semantic" characteristics	Level 1	Level 2
Grammatical characteristics	Level 3	Level 4

FIG. 1. Schematic Systematization of the Whorfian Hypothesis

characteristics of a given language or languages and the other pertaining to behavior of the speakers of the language or languages under consideration. The first factor has been dichotomized so as to distinguish between lexical or semantic structure on the one hand (both of these being considered as codeability features) and grammatical structure on the other. The second factor has been dichotomized so as to distinguish between verbal behavior per se (frequently interpreted in terms of cultural themes or *Weltanschauungen*) and individual behavioral data which is other than verbal in nature.

In a rough way, we might say that Levels 1 and 3 are concerned with *large group phenomena* whereas Levels 2 and 4 are concerned with *individual behavior*. Whorf was aware of and interested in both kinds of data, for he held that "our linguistically determined thought world not only collaborates with our *cultural idols and ideals* but engages even our unconscious *personal reactions* in its patterns and gives them certain typical character(istic)s" (1941, my italics).

In general, Whorf is not deeply concerned with "which was first, the language patterns or the cultural norms?" He is content to conclude that "in the main they have grown up together, constantly influencing each other" (1941).[5] Nevertheless, he does state that if these two streams are to be separated from each other for the purposes of analysis he considers language to be by far the more impervious, systematic, and rigid of the two. Thus, after a long association between culture and language, innovations in the former will have but minor impact on the latter, "whereas to inventors and innovators it (i.e., language) legislates with the decree immediate" (1941). Although Whorf is leary of the term correlation it seems most likely that he considered language structure not only as interactingly reflective of "cultural thought" but as directly formative of "individual thought." With proper cautions, the four levels of the Whorfian hypothesis that have been differentiated in this review may be seen as quite consistent with this conclusion.

Some of the characteristics, difficulties, and potentials of further empirical and theoretical study at each of the four differentiated levels have been considered. All levels can make use of both interlinguistic or intralinguistic designs, although Levels 1 and 3 most commonly employ the former—if only for purposes of contrast.

Although evidence favoring the Whorfian hypothesis exists at each level, it seems likely that linguistic relativity, though affecting some of our cognitive behavior, is nevertheless only a moderately powerful factor and a counteractable one at that. Certainly much experimental evidence has accumulated that points to a large domain of contra-Whorfian universality in connection with the relationships between *certain* structures of particular languages and *certain* cognitive behaviors of their speakers (see Osgood, 1960, e.g.). The time might, therefore, now be ripe for putting aside attempts at grossly "proving" or "disproving" the Whorfian hypothesis and, instead, focusing on attempts to delimit more sharply the types of language structures and the types of non-linguistic behaviors that do or do not show the Whorfian effect as well as the degree and the modifiability of this involvement when it does obtain.

Because of Whorf's central role in making us aware of this phenomenon so that we may now better come to grips with it, both intellectually and practically, none can deny that he richly deserves to be characterized by his own standard for what constitutes a real scientist.

> "All real scientists have their eyes primarily on background phenomena in our daily lives; and yet their studies have a way of bringing out a close relation between these unsuspected realms . . .and. . . foreground activities." (1940)

[5] Sapir also takes this view in his article on language and environment (1912), in which he concludes that grammatical features once did correspond to environmental circumstances but have subsequently developed at a pace and in directions unrelated to environmental changes.

REFERENCES

Bartlett, F. C. *Remembering*. London: Cambridge, 1932.

Boas, F. *Handbook of American Indian languages, Part* 1. Washington, D. C.: Government Printing Office, 1911.

Boas, F. (Ed.) Language. In *General anthropology*. Boston: Heath, 1938.

Brown, R. W. & Lenneberg, E. H. A study in language and cognition. *J. abnorm. soc. Psychol.*, 1954, 49, 454–462.

Carmichael, L., Hogan, H. P., & Walter, A. A. An experimental study of the effect of language on the reproduction of visually perceived form. *J. exp. Psychol.*, 1932, 15, 73–86.

Carroll, J. B. (Ed.) *Language, thought and reality; selected writings of Benjamin Lee Whorf*. New York: Wiley, 1956.

Carroll, J. B. and Casagrande, J. B. The function of language classifications in behavior. In Maccoby, Newcomb, & Hartley (Eds.), *Readings in social psychology*. New York: Holt, 1958.

Casagrande, J. B. Personal communication. March 18, 1960.

Ebbinghaus, H. Über eine neue Methode im Prüfunt geistiger Rühigkeiten und ihre Awendung bei Schulkindern. *Z. Psychol.*, 1897, 13, 401–457.

Eidens, H. Experimentelle Untersuchungen über den Denkuerlauf bei unmittelbarren Folgerungen. *Arch. ges. Psychol.*, 1929, 71, 1–66.

Fishman, J. A. An examination of the process and function of social stereotyping. *J. soc. Psychol.*, 1956, 43, 27–64.

Fishman, J. A. Some current research needs in the psychology of testimony. *J. soc. Issues*, 1957, 13, 60–67.

Galton, F. Psychometric experiments. *Brain*, 1879, 2, 149–162.

Gastil, R. D. Relative linguistic determinism. *Anthropological Linguistics*, 1959, 1, No. 9, 24–38.

Gates, A. I. Recitation as a factor in memorizing. *Arch. Psychol.*, 1917, 6, No. 40.

Glenn, E. S. Paper presented at the American Psychological Association meeting, Cincinnati, 1959.

Hallowell, A. Cultural factors in the structuralization of perception. In Rohrer & Sherif (Eds.), *Social psychology at the crossroads*. New York: Harper, 1951.

Hockett, C. F. Chinese versus English: an exploration of the Whorfian theses. In H. Hoijer (Ed.), *Language in culture*. Amer. Anthropological Ass., Memoir 79, 106–123, 1954.

Hoijer, H. Cultural implications of some Navaho linguistic categories. *Language*, 1951, 27, 11–120.

Hoijer, H. The Sapir-Whorf hypothesis. In H. Hoijer (Ed.), *Language in culture*. Amer. Anthropological Ass., Memoir 79, 92–104, 1954.

Kilpatrick, F. P. Perception theory and general semantics. *Etc.*, 1955, 12, 257–264.

Lee, D. D. Linguistic reflection of Wintu thought. *Int. J. Amer. Linguistics*, 1944, 10, 181–187.

Lehmann, A. Über Wiedererkennen. *Philos. Stud.* (Wundt), 1889, 5, 96–156.

Lenneberg, E. H. Cognition in ethnolinguistics. *Language*, 1953, 29, 463–471.

Lenneberg, E. H. A probabilistic approach to language learning. *Behav. Sci.*, 1957, 2, 1–12.

Lindeman, R. *Der Begriff der Conscience im Französichen Denken*. Jena-Leipzig, 1938.

Luria, A. R. The directive function of speech in development and dissolution. Part I: Development of the directive function in speech in early childhood. *Word*, 1959, 15, 341–352.

Lyon, D. O. The relation of length of material to time taken for learning and the optimum distribution of time. *J. educ. Psychol.*, 1914, 5, 1–9, 85–91, 155–163.

Maier, N. R. F. Reasoning in humans. I. On direction. *J. comp. Psychol.*, 1930, 10, 115–143.

Malinowski, B. The problem of meaning in primitive languages. In Ogden & Richards, *The meaning of meaning*. New York: Harcourt-Brace, 1923.

Miller, G. A., Bruner, J. S. & Postman, L. Familiarity of letter sequences and tachistoscopic identification. In G. A. Miller, *Language and communication*. New York: McGraw-Hill, 1951.

Miller, G. A. and Selfridge, J. A. Verbal context and the recall of meaningful material. *Amer. J. Psychol*, 1950, 63, 176–185.

Osgood, C. E. The cross-cultural generality of visual-verbal synesthetic tendencies. *Behav. Sci.*, 1960, 5, 146–169.

Piaget, J. *Le language et la pensée chez l'enfant*. Neuchâtel and Paris: Belachaux et Niestle, 1924.

Postman, L. & Conger, B. Verbal habits and the visual recognition of words. *Science*, 1954, 119, 671–673.

Postman, L. & Rosenzweig, M. P. Perceptual recognition of words. *J. Speech and Hearing Disorders*, 1957, 22, 245–253.

Reed, H. B. Repetition and association in learning. *Ped. Sem.*, 1924, 31, 147–155.

Sapir, E. Language and environment. *Amer. Anthrop.*, 1912, n.s., 14, 226–242.

Sapir, E. The status of linguistics as a science. *Language*, 1929, 5, 207–214.

Sapir, E. Language. *Encycl. Soc. Scs.*, 1933, 9, 155–169.

Sells, S. B. The atmosphere effect. *Arch. Psychol.*, 1936, 29, No. 200.

Solomon, R. L. & Howes, D. H. Word frequency, personal values, and visual duration thresholds. *Psychol Rev.*, 1951, 58, 256–270.

Trager, G. L. The systematization of the Whorf hypothesis. *Anthrop. Ling.*, 1959, 1, No. 1, 31–35.

Tsao, J. C. Studies in spaced and massed learning: II. Meaningfulness of material and distribution of practice. *Quart. J. exp. Psychol.*, 1948, 1, 79–84.

Vigotsky, L. S. Thought and speech. *Psychiatry*, 1939, 2, 29–54.

Whorf, B. L. Science and linguistics. *Technology Review*, 1940, 44, 229–231, 247, 248.

Whorf, B. L. The relation of habitual thought and behavior to language. In L. Spier (Ed.), *Language, culture and personality*. Menasha, Wisc.: 1941, Pp. 75–93.

Wilkins, M. C. The effect of changed material on the ability to do formal syllogistic reasoning. *Arch. Psychol.*, 1928, 16, No. 102.

Woodworth, R. S. & Sells, S. B. An atmosphere effect in formal syllogistic reasoning. *J. exp. Psychol.*, 1935, 18, 451–460.

(Manuscript received May 17, 1960.)

. . . Actually reason is very rarely, if ever, pure. Impulse is seldom totally blind. We are creatures of more or less intelligent impulses and more or less emotionally befuddled intelligence. "Pure reason" and "blind impulse" are ideal limits of abstraction from the real business of living. They are like those useful, indeed indispensable, fictions that serve as the elements of geometry—points of no size at all, lines with no breadth, planes without thickness. Or we might compare them to the asymptotes of a hyperbola, which the curve—in this case life itself—approaches continually but never quite reaches.

WILLIAM H. ROBERTS, *The Problem of Choice*

BOOK REVIEWS

Three Related Books

International conference on information processing. New York: UNESCO Publications Center, 1960. Pp. 640.

Mechanisation of thought processes. National Physical Laboratory, Symposium No. 10. London: Her Majesty's Stationery Office, 1959. Pp. 964.

M. C. Yovits and S. Cameron (Eds.). *Self-organizing systems.* New York: Pergamon Press, 1960. Pp. 322.

THE past five years have seen a major effort to put into operation many of the ideas, possibilities, and hopes of cybernetics. People in a number of traditionally unrelated disciplines—chiefly logic, mathematics, electrical engineering, linguistics, neurophysiology, psychiatry, and psychology—have separately and, occasionally, in uneasy coalition, made a surprising number of attacks against a group of superficially unrelated but almost certainly intimately interconnected and overlapping problems. The thing in common seems to be simply the getting of information-processing machines to make better use of information. The machines in question are conglomerations or nets of switches, in the concrete guise of general purpose digital computers, analog computers, or special purpose logical circuitry. But more often than not the designer of the machine feels he is really designing a network of neurons, or a hierarchical control system, or a theory of automata, or a psychology of discovery. The practical problems that have been attacked are storage and retrieval of information, as in a library, inventory control system or documentation center, translation of natural languages, recognition of patterns (printing, spoken words, photographs), transformation of information (as from mathematical to computer-programming notation, or analog to digital), and even mechanization of mathematics. Around these have cropped up what must certainly be among the most fascinating problems in the disciplines involved. For very quickly language translation becomes a problem of "meaning," pattern recognition of "perception" and "concept-formation," information retrieval of "description" and "remembering." Machines that can learn, organize themselves, take advice, imitate, discover, make decisions, make guesses are commonly suggested as the methods to speed up progress toward solving these practical problems.

The titles of recently published symposium papers of three groups summarize this core of interest. "Mechanisation of Thought Processes" (MTP) probably most adequately covers the range of problems and interests involved, with an active interplay between workers of different sorts (chiefly psychologist and neurophysiologist as opposed to engineer and applied mathematician) who have not as yet, but appear to want to have, gotten together. "Self-Organizing Systems" (SOS) reports a similar meeting. People of much more firmly established reputations in their own fields than their generally much younger British counterparts, the behavioral scientists at the Chicago conference (SOS) seem less interested in the relevance of their material to the problems at hand. So, although several of these papers are quite good as introductions to "what you might find worth knowing about my field," possibly only Milner's came from someone for whom the problems of the symposium were personally alive. In contrast, the British meeting heard and actively discussed papers by Whitfield, H. B. Barlow, Sutherland, and others that read as though the author, albeit from a very different starting point, is moving toward the same goal. The "International Conference on Information Processing" (ICIP) concentrated much more heavily on computer technology and programming problems though a number of papers (many of which were abstracted in *Behavioral Science*, 1959, 4, 342–351) presented computer work on artificial intelligence. Minsky and MacKay (MTP) present

provocative discussions that are in the way of notes toward a theory of "heuristic" methods—really a psychology of discovery and creative intelligence. Many other papers (for example, by Uttley, Rosenblatt, Andrew, Selfridge, and Taylor in MTP; by Farley, Rosenblatt, and Newell, Shaw, and Simon in SOS; and by Gelernter, Stevens, and Kilburn, Grimsdale, and Sumner in ICIP) report the results of computer programs for pattern recognition, learning, and problem-solving that embody certain "heuristic" or "self-organizing" features. But it seems extremely difficult to judge the results. Their promise, and the understanding they give us about heuristics, seem encouraging. But it appears that most if not all of the practical accomplishments can be done at least as well, and often with great simplifications, by more straightforward methods.

Language translation, pattern recognition, and automatic programming all seem to be specific instances of the general problem of forming common classes (with respect to certain properties which themselves must often be discovered) and mapping these classes from one domain to another. Here again much work has been done at a very practical, problem-oriented level and many programs with a certain amount of power to act upon a specific well-examined domain (such as a simplified "basic English" type language or a predetermined set of patterns) have given promising, even intriguing, results. But it is difficult to compare these results or to evaluate the eventual power of these approaches without a general conceptual framework. And appropriate theories of language might throw searching light on all these problems. A good bit of mathematical machinery might be brought to bear on these problems, once they are posed, at a suitably abstract level. And certainly their common source—the human beings who create the symbols and sets (languages) and notice the patterns (percepts) that the machines must cope with—should be examined by the behavioral scientist for the basic operators that they may use.

Although the computer realizations of attempts to do colorful and amazing things seem the most attractive and intriguing aspect of this enterprise, the development of basic theories of automata (including logical systems, languages, and neural nets) and of discovery (including self-organization), possibly best represented in these symposia by the work of Burks (SOS), McCulloch (MTP, SOS), MacKay and Minsky (MTP), may well turn out, in the long run, to be the most important contributions.

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George A. Miller, Eugene Galanter, and Karl H. Pribram, *Plans and the structure of behavior*. New York: Henry Holt, 1960

THIS stimulating, wide-ranging book deals with the relation between a creature's images of the world and its plans for action. The authors' task is "to explore the relation between the 'image' and the 'plan'." A plan is "*any hierarchical process in the organism that can control the order in which a sequence of operations is to be performed.*" Plans are found in all living systems, even if they be instinctual and their components are subject to little rearrangement (Ch. 5). However, the chief instrument of plans is language, and the authors analyze as human plans: intentions (Ch. 4), motor skills (Ch. 6), personality traits (Ch. 9), remembering (Ch. 10), speaking (Ch. 11), problem solving (Ch. 12), and the possible planning centers of the brain (Ch. 14).

The building blocks of plans (Ch. 2) are feed-back loops, well known in cybernetic theory, each of which contains a "test" to which the organism accommodates an input. If the accommodation is insufficient, it "operates" on the input, retests, and continues this process until it is enabled to "exit" from this cycle and transfer active control to another such test-operate-test-exit loop. These "TOTEs" may contain one another as components which permits a TOTE hierarchy to be constructed. This is a plan. A TOTE is clearly derived from work with computers which are being used to simulate goal seeking behaviors. The authors show, further, that it is also a generalization of the notion of reflex arc as well. But the reflex and its components,

stimulus and response, they reject as an inadequate unit for plans. In situations more complex than bar pressing, it is not at all clear whether stimulus and response can be specified. If they are taken to be any physical event impinging on the organism and any behavior, then most behavior is not lawful. If they are restricted only to correlated events, most of the organism's activity will not be considered behavior.

The only flow in a reflex is energies and thus the only tests are thresholds. But a TOTE may be the carrier of energies, information, or control, and the tests associated with these are much more numerous and resemble the tests organisms actually apply.

The authors are at pains to show in different contexts why a model of the organism containing only its past states without a representation of its plans for the future is inadequate. Motivational phenomena like resumption and recall of interrupted tasks studied by Lewin and his students require that "interruption" be given a meaning. This is only possible by specifying portions of a task not yet performed but planned. An intention is, in fact, "the uncompleted parts of a plan whose execution is already begun." They propose that since plans have structural characteristics (Ch 9: source, span, detail, flexibility, speed, coordination, retrieval, openness, stop orders), they can be specified more clearly than intentions. If these characteristics can all be derived as sibling TOTE structures, a way will be opened to translate the concepts of motivaton into structures which can be simulated.

Speaking, too, requires a plan to generate sentences. Any solely historical model of grammar will not do. Consider, for example, a model which regards the speaker as a finite state Markov process, emitting a symbol with each state-to-state transition. The authors follow the linguist Noam Chomsky in showing first, that it would take an enormous childhood to be exposed to enough sentences of reasonable length to learn the transition probabilities from state to state, and second that our language contains many hierarchical embedding devices which are productive, and yield, e.g.: "The man who said it wouldn't rain is coming today." In this example some plan must guide the composition of the clause within the total sentence. Motor skills, too, require plans, since "if skills were nothing but chains of reflexes, a detailed account of the correct sequence should be an efficient way to teach them." But just the opposite is true, for "the ability to develop tactics for one's self is a test of the adequacy of a strategy."

Probably the most dramatic application of the plan to behavior is the discussion of hypnosis . . . "relinquishing the plan" (Ch. 8). Hypnosis is a state where the person has suspended his own plans and follows the plans of another. A group of symptoms, including slow reluctant speech, often accompany hypnosis, indicating that internal speech is very important to forming plans, just as it is when people speak aloud in solving problems. Further, there seems to be a suspension of certain customary stop-rules, as shown in the subject's increased tolerance for pain. And finally, plans can be learned in hypnosis which require execution later.

There is a concluding discussion of the brain as a center of imaging and planning. The innerbrain, the frontal and limbic systems, controls the serial organization of behavior, and regulates central nervous activity. This is a planning center. The outerbrain, the sensory projection and association areas, controls the discriminative choice of signals from the outer world.

There are interesting suggestions in almost every chapter by these "subjective behaviorists" as the authors call themselves. The book touches on many topics but digs into few. There is much mention of TOTEs, but there is no case of a worked out TOTE plan for any of the many phenomena mentioned. Again, their recasting of the will as a plan seems very attractive, but they give no clear predictions which depart from Lewin's. There are a few cursory remarks about social planning. The book as a whole is an illuminating "image," which deserves a "plan" to go with it.

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COMPUTERS IN BEHAVIORAL SCIENCE

The Digital Simulation of Neuro-Muscular Organisms,

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INTRODUCTION

In recent years there have been several digital simulation studies of neural-like network models (Crichton & Holland, 1959; Farley & Clark, 1954, 1955; Frankel, 1955; Rochester, Holland, Haibt, & Duda, 1956). The authors of these studies are, for the most part, mathematicians and engineers. Their natural inclinations and background make them singularly receptive to the notion that a nervous system, at least at the higher levels, is essentially a logic machine resembling in many ways the digital computers of today. This notion was made fashionable by such theorists as McCulloch (1943, 1949a, 1949b, 1951, 1957), Culbertson (1950, 1956), Rashevsky (1948), Kleene (1956), Wiener (1948), and the late John von Neumann (1951, 1956). It should be noted, however, that in his last book (1958) von Neumann did a rather thorough job of discrediting the computer analogy.

The behavioral scientist who takes the trouble to examine these theories and simulation studies might justifiably become annoyed by the glib assumptions and idealizations he finds. After comparing, for example, the McCulloch neuron model with the brief survey of physiological evidence recently published by Bullock (1959), one might reasonably ask whether these gentlemen are talking about the same physical processes. But the digital simulation studies to date deserve attention, however unrealistic may be the models upon which they are based. They represent pioneer efforts to develope digital simulation as a powerful theoretical tool for the study of complex, organized ensembles of active elements. Such ensembles and systems, especially when they contain hierarchies of non-linear elements and discontinuous processes, cannot be effectively represented by tidy, conventional, mathematical models (see Weaver, 1948). The behavioral scientist who approaches the subject of neural simulation for the first time is advised to read the excellent little IRE symposium of 1956; I would direct the reader's attention particularly to the statements of Otto H. Schmitt.

As a point of departure for the present paper, several possible criticisms of previous simulation studies are listed below:

1. The disparity between neural models and physiological evidence is too great; the living nervous system does *not* resemble a synchronous, logical computer. Furthermore, the authors do not seem concerned by the effects of this disparity on the validity of any results obtained.
2. The model networks are too isolated; they do not *interact* with a model environment. Both physiological and psychological evidence suggests that such interaction plays a crucial role at all levels of behavior in living organisms.
3. The subject of these studies is the central nervous system. While one cannot condemn direct attacks on the most difficult problem first, the history of science would seem to indicate that successful solutions of very difficult problems are preceded by a great deal of hard work on simpler sub-problems together with a methodical sharpening of theoretical tools. Is it likely that a sound theory of the central nervous system can ever be achieved without first developing a precise theory of the lower and simpler peripheral mechanisms? And simulation models of even these simpler biological systems must

[1] This is a somewhat revised and enlarged version of an invited paper presented at the 16th National Conference of the Operations Research Society of America at Pasadena on November 11, 1959.

be dangerously idealized in order to meet the economic and technical limitations of present-day computers.

4. It appears that there has been little active participation on the part of biologists in the formulation or use of simulation models. While it is true that as yet few biologists are sufficiently acquainted with digital computers to materially influence the creation of simulation models (except to veto idealizations), every effort should be made to elicit their cooperation in the formulation and execution of simulation experiments.

We believe that these are valid criticisms, at least for those who are interested in real nervous systems, and have taken them into account in planning the simulation model described in this paper. It will become all too apparent, however, that we have been able to only partially avoid these criticisms. We view this program as being a step, but only a step, toward powerful simulation programs for the study of real biological systems.

The body of this paper is divided into two sections. The first section contains a description of a particular simulation model and some comments on the general problems connected with simulation studies. This section does not require, on the part of the reader, any detailed knowledge of computer programming.

The second section describes certain technical features of the simulation program designed for the above mentioned model. To the author's knowledge, these features have not been incorporated in any previous simulation programs, and since they provide important advantages in economy and flexibility they may be of general interest to others engaged in simulation studies.

A SIMULATION MODEL AND RELATED PROBLEMS

In selecting a neural model, the major premise was that neurons are fundamentally analog devices, i.e., they measure rather than count. The fundamental scale by which the neuron measures its environment is frequency of firing, which can vary continuously over some range determined by the

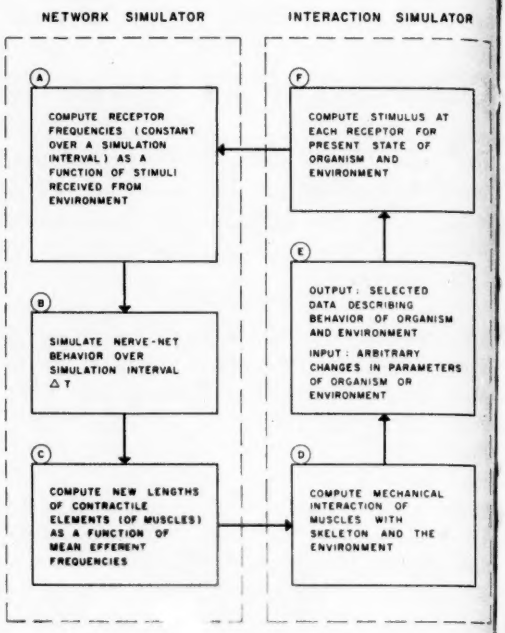
NETWORK SIMULATOR
INTERACTION SIMULATOR
A
COMPUTE RECEPTOR FREQUENCIES (CONSTANT OVER A SIMULATION INTERVAL) AS A FUNCTION OF STIMULI RECEIVED FROM ENVIRONMENT
B
SIMULATE NERVE-NET BEHAVIOR OVER SIMULATION INTERVAL Δ T
C
COMPUTE NEW LENGTHS OF CONTRACTILE ELEMENTS (OF MUSCLES) AS A FUNCTION OF MEAN EFFERENT FREQUENCIES
D
COMPUTE MECHANICAL INTERACTION OF MUSCLES WITH SKELETON AND THE ENVIRONMENT
E
OUTPUT: SELECTED DATA DESCRIBING BEHAVIOR OF ORGANISM AND ENVIRONMENT
INPUT: ARBITRARY CHANGES IN PARAMETERS OF ORGANISM OR ENVIRONMENT
F
COMPUTE STIMULUS AT EACH RECEPTOR FOR PRESENT STATE OF ORGANISM AND ENVIRONMENT

FIGURE 1

physical state of the neuron. Thus we might consider a nervous system to be a "pulsed-analog" machine.

A second major premise was that present factual knowledge and theoretical tools are quite inadequate for understanding the vertebrate central nervous system. Consequently we decided that the soundest course of action is to begin by studying the simplest types of neuro-muscular mechanisms, such as are found in the invertebrates and the peripheral reflexes of the vertebrates, with the ultimate objective of developing a quantitative theory of structure-function relationships. If success attends these efforts, and as techniques of simulation are improved, it may be possible to slowly climb up the evolutionary scale, advancing beyond any given level only when mechanisms at that level are well understood. We seriously doubt that there are any royal roads to understanding the central nervous system. Living organisms, even at the level of the amoeba, are incredibly complex, far more so than the systems analyzed by physicists. This conservative, evolutionary approach therefore seems to us the most sensible.

A third premise was that the significance

of any element in a mechanism can be properly understood only in its relation to the behavior of the total mechanism. Since the lower mechanisms are intimately related to the control of muscles for manipulating the environment or moving about, it is essential that we simulate the physical interaction of the whole organism with the environment, not just isolated nerve-nets.

Figure 1 is a general flow chart of a simulation program. It shows the sequence of processes involved in simulating an "organism." These processes are divided into two groups and are carried out by two distinct programs: the Network Simulator and the Interaction Simulator.

The Network Simulator is a general purpose program that can simulate any network (within certain limits) composed of the "elements" described below. To set up a particular network one simply feeds in a list of desired elements and connections. This program has been coded for the IBM 709 computer and is the main subject of the second section of this paper.

Unfortunately we have been unable to create a flexible, general purpose model of physical interactions between organisms and environments. For example, we know of no general, practical, mathematical model for simulating the behavior of a system of rigid linkages (such as occur in skeletons). The only feasible technique at present is to work out a particular model for a particular organism. Consequently it will usually be necessary to program a special Interaction Simulator for each organism to be studied. However, most physical interactions can be represented by rather conventional mathematical functions, and the easily learned, powerful FORTRAN programming language is designed for just such functions. The Interaction Simulator can therefore be programmed in FORTRAN. A biologist, for example, can get a fair grasp of FORTRAN programming in two or three days. With a modicum of aid from a professional programmer, he could write an Interaction Simulator to suit his taste. We intend to provide such aid to any academic user in the Los Angeles area.

Boxes D, E, and F in Figure 1 represent the three main processes generally required in an Interaction Simulator. On the assump-

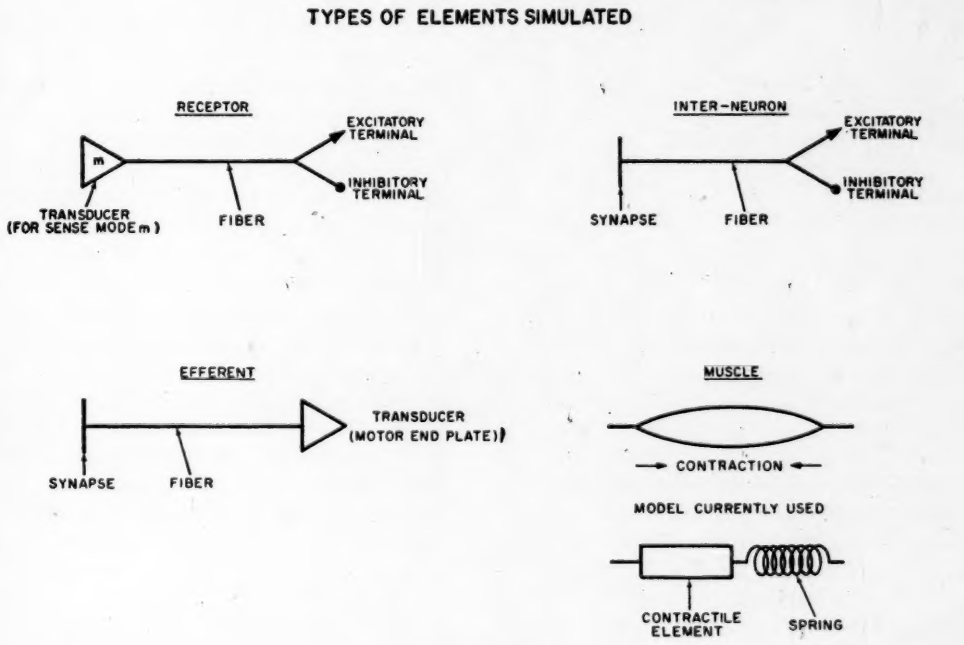

FIGURE 2

tion that this program will always be programmed in FORTRAN language, the Network Simulator has been designed so that it can be easily linked with any FORTRAN program.

Figure 2 shows the four types of "elements" that can presently be simulated by the Network Simulator. The graphical representations used here are based partly on the diagrams of Culbertson (1950) and partly on conventional electronic schematics. The names of these elements will be capitalized in order to distinguish them from the corresponding physiological elements they represent.

The "Receptor" is treated as an element that converts some physical state of the environment into a corresponding frequency of firing, i.e., it is a transducer. A symbol in the triangle designates the sense mode of the particular Receptor, e.g., tension, length, temperature, etc. Two kinds of output terminals are possible, excitatory and inhibitory, and to have any effect on the organism, they must lie on the "synapses" of other elements. An interpolation table is used to determine the firing frequency as a continuous function of physical state. At the present time these tables each provide ten points. Linear interpolation is used between points.

The "Inter-neuron" has a single "synapse" and one or more excitatory and inhibitory output terminals. The synapse model currently used is described in some detail below.

The third type of neuron is the "Efferent." Its output is a transducer that converts frequency of firing into contraction of the muscle to which it is connected. The amount of contraction is a continuous function of frequency and is computed from a tenpoint interpolation table. If more than one Efferent is connected to a single muscle, then all of those Efferents must be assigned the same interpolation table and the mean frequency is used to compute contraction.

The fourth type of element is the "Muscle." At the present time, the muscle model consists of a contractile element in series with a passive spring. The length of the contractile element is entirely determined by the Efferents connected to it. The length of the spring, and therefore the force exerted by the muscle, is determined by the length of the contractile element and the various static and dynamic forces of the skeleton or environment to which the muscle is attached.

A most important feature of the Network Simulator is its ability to simulate completely asynchronous firing in the network elements. The firing frequency of each Receptor is independently determined by the state of the environment; no two Receptors need have the same frequency. And since the frequency of a Receptor is effectively a *continuous* variable, the phase relations of Receptor pulses arriving at the synapse of an Inter-neuron or Efferent can vary continuously from zero up to several seconds. This asynchrony, further increased by independent parameters of Inter-neurons, will propagate throughout the network. Thus the main behavioral variable of an element is its *mean firing frequency* over some period of time, and it is a *continuous* variable. This is in line with our assumption that real neurons are essentially analog measuring devices rather than digital, logical bistable flip-flops.

The present Network Simulator permits a maximum of 50 Receptors, 250 Interneurons plus Efferents, and 20 Muscles. There is no fixed limit to the number of output terminals a particular Receptor or Inter-neuron may possess. However, the total number of output terminals in a network should not far exceed 1000. This is only a rule of thumb; the actual limit on the number of terminals depends on the maximum possible activity of a given network and the length of the simulation interval. There is no limit whatsoever on the number of terminals that may contact the "synapse" of a particular element. So long as these restrictions are met, the Network Simulator can simulate any combination of the four types of elements connected together in any way. In fact two or more *separate* networks can be simultaneously simulated with ease if the total numbers of elements and terminals are less than the limits posted above. The type of organism

for which this program is intended will generally require less than 100 elements.

Time limitations are far more important than size limitations. The "simulation rate" is defined as the ratio of the "lifetime" of the organism to the machine time required to simulate that lifetime. If the simulation rate is 1/10, then on the average 10 minutes of machine time will be required to simulate 1 minute in the life of the organism. The cost of a simulation experiment in time and money depends on the simulation rate; the higher the rate, the lower the cost. If the simulation rate is 1, the simulation is said to be in "real time." But this is difficult to attain with systems of any complexity.

The simulation rate of the present Network Simulator is a function of the number of output terminals in the network plus the "activity" of the net, i.e., the number of pulses propagating along axons at the moment. The number of elements is irrelevant. Since the activity of a network will in general be fluctuating, perhaps over a wide range, with the passage of time, the simulation rate will likewise vary; the greater the activity, the lower the simulation rate. On the basis of debugging experiments, it appears that the present program cannot achieve real time simulation with networks composed of more than a dozen single-terminal elements firing at frequencies in the neighborhood of 100 pulses per second.

The over-all simulation rate will always be less than that of the Network Simulator because the mechanical interaction of the organism with the environment must also be computed at regular intervals. This interval will be called the "simulation interval" and is defined in terms of seconds in the life of the organism. If for example, the simulation interval is 5, then every 5 seconds in the life of the organism time stops and the effect of Muscles on environment and environment on Receptors is computed. Boxes C, D, E, F, and A in the flow chart of Figure 1 represent the component programs that compute the interaction. In addition to muscular changes arising from Efferent activity over the previous simulation interval, the Interaction Simulator must take into account inertial forces extrapolated from previous interactions. Since real organisms interact continuously with their environment, it would be desirable to simulate this interaction at very short intervals. Unfortunately, this desire is in direct conflict with the economic goal of maintaining a high simulation rate. Time stands still for the organism while environment interaction is being computed, thereby reducing the over-all simulation rate.

Suppose we let T_n represent the average machine time required to simulate one second of network activity (carried out by box B in Figure 1). This remains constant no matter how often the environment is sampled. Let T_e be the machine time required to compute the organism-environment interaction at any particular instant, and Δt be the simulation interval, i.e., the time (in the organism's life) between computations of environmental interaction. The simulation rate R is represented by the function

$$R = \frac{\Delta t}{T_n \Delta t + T_e}.$$

If we take, for example, a modest organism where $T_n = 2$ sec. and $T_e = 1$ sec., we find that a simulation interval of 1 sec. gives a rate of 1/3, an interval of 0.1 sec. cuts the rate to 1/12, and an interval of 0.01 sec., which would provide a very close approximation to continuous interaction for most organisms, drops the simulation rate to less than 1/100, i.e., more than an hour and a half would be required to simulate one minute in the life of the organism.

Thus the experimenter must choose a compromise between simulation *rate* and simulation *validity*. Of course this compromise plays a major role throughout the process of creating a model and designing the corresponding simulation program. Idealized and simplified models improve the simulation rate at the expense of empirical validity. But then the trade of precision for economy is an important facet of theory construction (i.e., the creation of models) in any field of science, although the economy is that of human thought and effort rather than machine time.

Although a synapse is technically a volume, however small, of extracellular material lying between the axon terminal of one (presynaptic) neuron and the soma or dendrite of another (postsynaptic) neuron, the Network Simulator treats the "synapse" as if it were a special part of the postsynaptic element. By attaching all synaptic parameters to the postsynaptic element, data processing by the machine is reduced without weakening the synaptic model.

Each Inter-neuron and Efferent possesses exactly one "synapse." No matter how many output terminals may be in contact with a given synapse, perfect *spatial* summation of incoming pulses is assumed. A "pulse" is treated as a quantity of energy unidirectionally transferred from an output terminal to the synapse on which that terminal lies. Each terminal in the network is uniquely assigned its own "pulse energy." If that energy has a negative sign, then the terminal is "inhibitory." The pulse energy, like all other parameters of the elements, is represented by a 35-bit number and can therefore be varied over a wide range of values.

The synaptic delay is not treated as a separate parameter, but is lumped together with the time required for conduction of a signal from synapse to output terminal. This is called the "transit time." Each Inter-neuron is assigned its own transit time, and this applies to all of the output terminals of that element. Since Receptors do not have a synapse and Efferents have no output terminals, these elements are not assigned transit times (see Figure 2).

The behavior of a synapse is determined by three parameters (see Figure 3) and the influx of energy from terminals contacting that synapse. The fundamental assumption is that the synapse is a place where *energy* is *stored*. The form of this energy is not defined and the units are therefore arbitrary. It is likely that a combination of chemical, thermal, mechanical, and electrical energy is involved in actual synaptic processes.

If the energy at a synapse is raised above the threshold θ by an incoming pulse from a terminal, then the synapse "fires," i.e., initiates an all-or-none signal that is propagated to all the terminals of that element. In the act of firing, the synapse loses all stored energy and passes into an absolute refractory state for a period R, during which all incoming pulses from terminals are lost. At the end of the refractory period the synapse begins again to store energy emitted by terminals. The energy emitted by "inhibitory" terminals has a negative sign and is added algebraically to the synapse energy already present. Thus the synapse energy can be driven below zero.

The use of an absolute refractory period is an oversimplification; it represents merely the frequency limitations of axon firing rather than a property of the synapse.

The third synaptic parameter is the leakage period L. This is the period required for all energy stored at the synapse, whether positive or negative, to "leak" away as a linear function of time. If a terminal emits a pulse of energy before the end of the leakage period, then that pulse is added to whatever energy remains and the leakage process

PARAMETERS OF ELEMENTS

Synapses:	θ	Threshold
	L	Leakage Period
	R	Refactory Period
Interneuron:	TT	Transit Time
Terminals:	E	Energy Emitted per Pulse
Muscles:	γ	Stretch Coefficient of Spring Element
Transducers:		Interpolation Tables for Converting Physical States to Frequency (Used by Receptors)
		Interpolation Tables for Converting Frequency to Length (Used by Muscles)

FIGURE 3

begins anew. Since L is a fixed interval of time, the leakage *rate* is directly proportional to the amount of energy initially present. We feel that this simple algorithm is a sufficiently good approximation to exponential decay for present purposes.

This leakage process permits the temporal summation of subthreshold pulses. Consider the case of a single excitatory terminal contacting a synapse. Suppose the pulse energy E of the terminal is less than the synapse threshold θ. If the terminal emits pulses at a fairly constant frequency f, then the synapse will never "fire" unless $f \geq 1/L$. Thus we have, in this case a "frequency threshold." If this threshold is exceeded by f, the firing frequency of the synapse is a rather complex function of f, θ, E, L, and R. Recalling that the elements of a network are generally firing asynchronously due to the cumulative effects of different transit times, independently varying Receptor frequencies, etc., it is readily apparent that the firing frequency of a synapse contacted by two or more terminals is not easily predicted. A separate simulation study of this synapse model is being carried out in our laboratory with the aim of finding a set of numerical functions describing its general behavior. Although it is simple, this model exhibits a surprising variety of firing functions for various ranges of the parameters.

Since the neurophysiologist usually measures voltages rather than energy, it is perhaps worthwhile to state the assumed correlation between those voltages and the parameters of our model. When the energy stored at the synapse is zero, the neuron membrane is at "resting potential." A positive synapse energy corresponds to "depolarization" and a negative energy to "hyperpolarization." The interrelations between terminal energies and θ, L, R represent, in effect, a "generator potential."

When we gain a better understanding of the present synapse model we intend to introduce, one by one, such refinements as multiple dendrites with decremental conduction, sinusoidal and sawtooth generators within the element, geometrical parameters for spatial summation, etc. Some of these refinements will require only minor modifications of the present program. The McCulloch type of logical, synchronous network can be simulated simply by assigning appropriate element parameters and Receptor interpolation tables, i.e., no modification is required.

To set up a particular network, the experimenter punches on standard IBM cards a list of the elements he desires, the parameter values, and the terminal connections. A special input program of the Network Simulator (not shown in Figure 1) reads this Description Deck and generates in the computer's memory a digital model of that network. If the experimenter sets a switch on the machine's console, the Network Simulator will automatically dump on magnetic tape, at the end of each simulation interval, a complete record of element parameters and variables (such as mean firing frequency of each neuron, lengths of contractile elements, etc.). The contents of the magnetic tape are printed out later, i.e., "off-line," on the high speed printer (500 lines per minute). Any additional inputs or outputs desired must be programmed in FORTRAN language as part of the Interaction Simulator.

We had hoped to use the Cathode Ray Tube display for presentation of selected variables during simulation. Unfortunately, the CRT display proved so unreliable it was dropped from the program.

In general, the over-all program (Network Simulator + Interaction Simulator) is designed so that it will run indefinitely. By means of the "on-line" printer the experimenter observes the behavior of the organism as it occurs, adjusting network and environment parameters via console sense and register switches. In this way the experimenter can get the "feel" of an organism much as a zoologist becomes generally acquainted with an animal by poking and prodding it in the laboratory.

The present Network Simulator has no built-in facilities for automatically changing parameters as a function of time or behavior in order to simulate "learning" processes. However, all parameters except output-terminal energies are organized into tables directly accessible to any FORTRAN program. If one wishes, for instance, to introduce facilitation (decrease in θ) as a

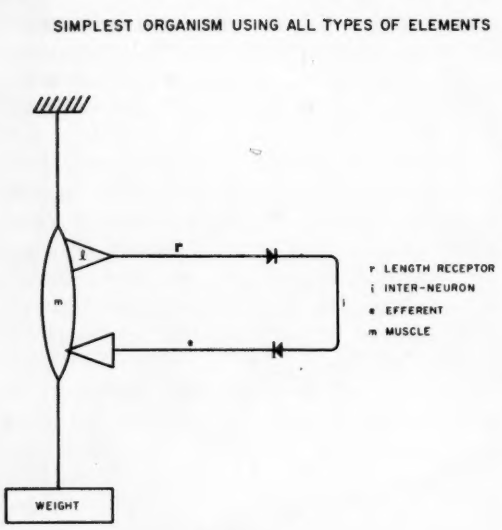

FIGURE 4

function of firing frequency, then the appropriate program is written in FORTRAN language and put in series with the Interaction Simulator. Thus all thresholds in the network could be modified at the end of each simulation interval according to the behavior of the corresponding synapses during that interval.

Figure 4 is a schematic diagram of the simplest possible organism that uses all four types of element. The environment is a weight supported by the Muscle M. The nerve-net consists of Receptor r, sensitive to Muscle length, with a single excitatory terminal on the synapse of Inter-neuron i, which in turn has a single excitatory terminal on the synapse of Efferent e. The mean firing frequency of e, over a simulation interval, determines the length of the contractile element in the Muscle at the end of the interval. The Interaction Simulator computes a new position and velocity of the weight on the basis of the new muscular force and previous positions and velocities. The Network Simulator computes the Receptor firing frequency for the next simulation interval and then proceeds to simulate the network over that interval. And so on indefinitely, i.e., until the experimenter stops the machine. It is important that the Interaction Simulator take account of several previous simulation intervals, otherwise the mechanical movements will be jerky and unrealistic due to poor approximations of changes in inertial forces. Of course the movements become smoother and more accurate as the simulation interval is decreased. As a matter of fact, the contractile elements themselves have a certain amount of inertia because the Network Simulator (at present) uses the mean Efferent firing frequency of *three* previous intervals for computing a new contractile element length.

The initial program debugging tests were carried out on organisms of the general type shown in Figure 4.

Figure 5 illustrates a somewhat more complex organism that is able to respond to a moving light source L which constitutes the environment. An "eye," composed of three photo-Receptors R_1, R_2, and R_3, is suspended between two antagonistic Muscles M_1 and M_2 which are distally attached to a rigid framework. In addition to the photo-Receptors, the network has as input two tension Receptors, R_4 and R_7, and two length Receptors, R_5 and R_6. The output of the network is the pair of Efferents E_1 and E_2 which control the contractile elements in the Muscles. The network contains eleven Inter-neurons, and there are several inhibitory terminals. The first organisms to be seriously studied will be of this general type.

Although this organism is elementary compared to those that could be simulated with the Network Simulator, we find that 77 parameters and 4 transducer tables are required to specify the network alone. This brings us to what is perhaps the most critical

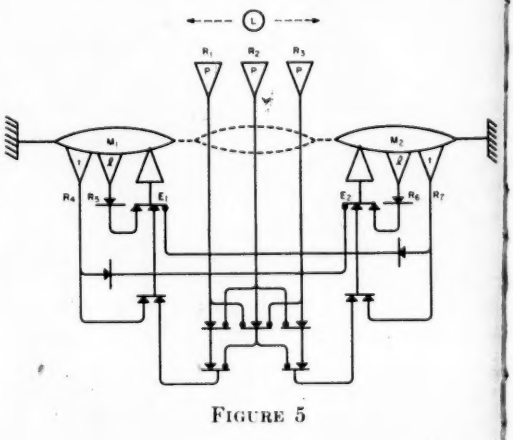

FIGURE 5

problem in the simulation of carefully *organized* systems, namely, the experimental control and observation of model behavior. We have already touched on the problems of machine-time economy and model validity. These two problems can be satisfactorily resolved for a rather rich variety of organisms, but their resolution is pointless if simulation models are beyond the practical control of humans. The great advantage of a simulation model is that it permits *perfect control* and *perfect observation*. But can we make *use* of this advantage, i.e., is it a *real* advantage?

In studying organism models of the sort envisioned in this paper, one is soon faced with thousands of parameters and variables. Conventional mathematical functions are not much help; if we could formulate a set of solvable system equations for the organism, there would be no need for a simulation model. The computer would be used simply to solve these equations by finite approximation techniques. Most scientific computation to date has been of this nature.

On the other hand, it appears that a chief characteristic of hierarchal networks of nonlinear elements with multiple feedback loops is that almost any parameter, under certain conditions, may have a great influence on the total behavior of the organism. One is *not* dealing here with large numbers of *homogeneously influential elements*, such as molecules in a volume of gas, and it seems that statistical control and analysis will therefore be of limited use.

Of course the behavioral scientist has been struggling with this problem for many, many years. However a major part of his effort has been directed toward the solution of the *instrumental* problems of empirical control and observation. When these instrumental problems are removed, as they are in simulation studies, the theoretical aspects of control and analysis come dramatically to the forefront.

It is not enough, then, to create economically feasible, empirically valid simulation models; we must also find some rational approach to the use of such models. We can do no more here than briefly describe *an* approach, one which calls for a minimum of philosophy and a maximum of work. Two parallel lines of research are involved.

First, a variety of organisms are to be studied, using simulation programs such as that described in this paper. For each particular organism, experiments will successively be aimed at four more or less distinct goals:

(1) *Identification* of influential parameters for various constellations of parameter values.

(2) *Relative weighting* of influential parameters for those constellations that yield satisfactory behavior of the whole organism.

(3) *Mathematical generalizations*, however fragmentary, of the relations between the most influential parameters and organism behavior.

(4) *Classification* of organisms, and substructures of organisms, on the basis of similar structure-function relationships. This goal is not achievable until goal (3) has been satisfied for several different organisms.

If and when goal (4) is reached, we are still a long way from our ultimate objective: a comprehensive, quantitative theory of complex organisms (whatever *that* is).

Of course, one can point out that the experimental goals listed above are present, at least implicitly, in all scientific research, and scarcely need listing. Agreed. But for reasons that will become apparent in a moment, it is essential that we pay close and conscious attention to the methodology of the experiments, and that the successive objectives be ordered by increasing difficulty of the inductive generalizations involved. The human scientist typically thinks at many levels with many goals and shifts from one to another with great facility. He is therefore not generally conscious of these continuous shifts of emphasis and level in his thought processes. But contemporary computers are not so facile or clever, and their capabilities must be improved in step-wise fashion with each new capability being first understood, clearly and explicitly, by a human programmer. This brings us to the other line of research.

Second, a series of special programs, which I shall call "Auto-Empiricists," are to be developed as the simulation studies proceed. An Auto-Empiricist is a program that shares the computer with a simulation program and stands, as it were, between the simulation program and the human experimenter. The role of an Auto-Empiricist might be likened to that of a laboratory technician or graduate student. It will work under the guidance of a scientist (human), preparing and executing experiments on the simulator, recording data, and organizing the data into preliminary tables, graphs, empirical functions, etc. The programmatic techniques required for these processes are already well known. But these processes simply provide the Auto-Empiricist with eyes and hands for playing with simulated organisms, and constitute just the first developmental step.

Our central objective in this line of research is to gradually improve an Auto-Empiricist so that it requires less and less guidance for more and more complex simulation models. It must be able to plan and execute whole series of experiments according to a few general criteria. And it must provide the human experimenter with significant inductive generalizations, not reams of numbers. If such programs can be successfully constructed, then it can truly be said that machines have become (at least) junior partners of man in scientific research.

Just how far and how fast the Auto-Empiricist project can be pushed only time will tell. Very little has been done on the mechanization of inductive inference and related thought processes (e.g. Solomonoff, 1957). Hopefully, the work of Newell, Shaw, and Simon (1958, 1959) and others on the mechanization of heuristic and deductive thinking will prove to be relevant and useful. Unfortunately, the philosophers of science have talked a great deal about induction, but have said little of practical value.

We are not concerned, however, with attempts to formulate a general theory of empirical induction from which one could *deduce* the necessary parts of an Auto-Empiricist. Rather, we are observing our own behavior in planning, executing, and analyzing the results of actual simulation experiments. Whenever we find ourselves repeatedly using a clearly definable pattern of action or analysis, we shall attempt to introduce that into the Auto-Empiricist. Thus the four successive experimental goals listed above are intended to provide a framework for self-observation as well as a practical guide for planning experiments. Needless to say, they are tentative and subject to change without notice as we gain experience. The first Auto-Empiricist is currently being programmed. It will control Synapse III, the third in a series of programs we are using for detailed studies of various synapse models. We shall not attempt to construct an Auto-Empiricist for the program described in this paper until it has been used for some time.

To summarize briefly, the Network Simulator represents an attempt to improve the validity of neural simulation studies by introducing a more realistic model. An evolutionary and holistic approach to the study of neuro-muscular systems has guided the design of this program. In simulating such complex, organized systems three main problems arise: economy of machine-time, effects of model idealizations on validity of molar behavior, and the control and analysis of many-parameter experiments. The first concerns primarily programmers, machine designers, and budget bureaus. The second problem suggests the necessity of close cooperation between interested biologists, psychologists, and those of us in the field of automata research. The third is a fundamental problem in all behavioral sciences: no matter how large, fast, and cheap computers become, digital simulation will never be a really powerful theoretical tool for the behavioral sciences unless sophisticated "Auto-Empiricists" are simultaneously developed. Thus there is a close relation between digital simulation and the mechanization of thought processes.

THE NETWORK SIMULATOR PROGRAM

In order to describe a program in any detail, even a relatively simple program such as the Network Simulator, one must unfortunately resort to elaborate flow charts

and ponderous prose. Thus we can do no more here than briefly touch on two features of the Network Simulator that may be of general interest. It is furthermore necessary to assume that the reader is familiar with programming terminology and ideas. Anyone engaged in simulation studies (of whatever sort) who desires more detailed information on this program should feel free to contact the author.

A member of our laboratory staff, Miss Alice Opolak, assumed the task of coding and debugging the Network Simulator. Most of it was coded in the SAP language, with FORTRAN being used in certain parts of the input/output processes and routines for linking the Interaction Simulators. Through the generous cooperation of the Western Data Processing Center, machine time for debugging was made available on their computer. A total of approximately two hours, scattered over many months, was required.

The complete program deck of the Network Simulator, together with instruction manuals, will be placed on file at the WDPC as part of their library. The program occupies about 1000 words in memory, and an additional 12,000 words are used for data storage.

The Network Simulator features to be described involve the use of "associative lists," a technique for storing and linking symbols that has been recently developed to a high degree by Newell, Shaw, and Simon (1957) and Shaw, Newell, Simon, and Ellis (1958). We shall use some of their terminology and notation in discussing these lists. It should be pointed out that the Network Simulator could have been programmed without the use of associative lists, but we wished to see if their use led to economies and increased flexibility. We are now satisfied that these lists have resulted in great savings in both machine-time and machine-space. We have not, however, used them in the subtle and complex forms found in the programs of Newell, et. al.

The device of "associating" two memory cells by storing the address of one cell in a field of the other cell has been used for many years. Many 2, 3, and 4 address machines, for example the IBM 650, use this device for linking together the successive instructions of a program. However, Newell, et al., were the first to recognize the advantages of using the principle of associative links for allocating and ordering both data and instructions. Their various Information Processing Languages offer beautiful examples of the powerful effects obtained by extensive use of this simple device.

In our simulation program, a very limited use of associative lists solved two major problems. First, in simulating an asynchronous network the program must scan the synapses in an unpredictable order determined entirely by previous synapse firings and the related transit time delays. If the program is to be at all efficient with large networks, it should examine a synapse *when and only when* a terminal emits a pulse at that synapse. Thus a table of future events requiring attention, i.e., the emission of terminal pulses, must be maintained. Preferably, these events are always listed in order of their occurrence in time, and since no two Receptors need fire at the same frequency nor any two Inter-neurons need have the same transit time, there is no general rule for predicting the location in that table of any given event. Consequently the table must continually be reordered. Furthermore, the length of the table constantly fluctuates as a function of the number of synapses firing. The programmatic problem of maintaining such a table becomes trivial if the table is in the form of an associative list.

A second problem is the efficient allocation of memory in the description of network topology. Each neuron has a fixed number of parameters, exclusive of the output terminals, and therefore a fixed-length block of cells can be set aside for each neuron. However, we wish to put as few limits as possible on the number of output terminals a given neuron may have. Thus one neuron may have two terminals and the next may have 40 terminals. If, say, a block of 50 cells were assigned to each neuron for listing its terminals, a major portion of the machine's memory would be consumed even though only a small fraction of this space is required for any particular network. By putting

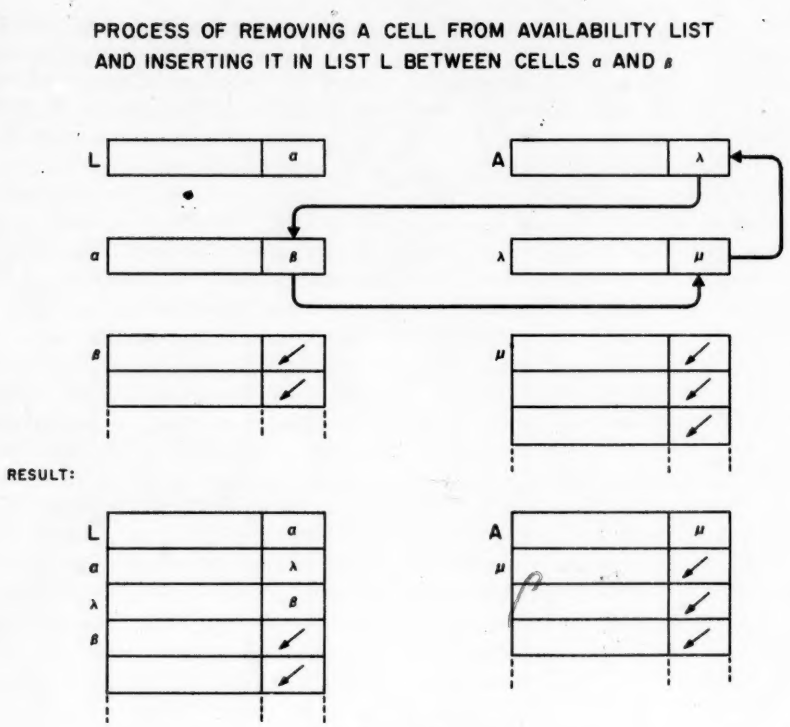

FIGURE 6

terminal lists in associative form, little space is wasted even though the range of possible terminals for a given neuron is not restricted.

Figure 6 illustrates the general structure of associative lists, and the process of removing a cell from one list and inserting it *between* two cells in another list. Each cell is represented by a long rectangle, with the address written on the left. A field at the right end of each cell contains the address of the next (associated) cell. The first cell of each list, called the "head," has a fixed address (L and A). Initially, all associative cells in memory are organized into a single list, called the Availability List. Whenever the program requires a cell for another list, it is obtained from the Availability List which serves as a reservoir of unused cells. This involves the shifting of only three addresses, as indicated by the long arrows. In the lower half of the figure is a picture of the final result from the programmer's point of view. The small arrows in the associative fields are a convenient shorthand notation indicating that a cell is linked to the cell immediately below it in the "list."

Similarly, when a cell is no longer required by a particular list, it is returned to the Availability List. Thus a program can easily do its own data allocation without human help. Furthermore, the *insertion*, *deletion*, or *reordering* of cells in a list can be performed by the transfer of a few addresses rather than the massive transfer of data.

Before reading in the cards describing a network, the Network Simulator organizes a "two-layer" Availability List with 4000 "entries." As used in this program, each entry in associative lists must hold more data than could be conveniently crammed into a single memory cell. Consequently, an "entry" in our associative lists is composed of *two* cells. The first cell (which has some address α) contains, in addition to data, the address of the "next," or associated, entry. A second cell, the "twin" of α, is uniquely defined; it is the cell whose address is $\alpha + 4000$. The twins constitute, as it were, a second "layer" of the associative lists. This is but one of several possible ways to expand the storage capacity of an entry.

Figure 7 illustrates the use of a two-layer

associative list to represent the connections and pulse energies of the four output terminals of Inter-neuron *a*. On the left is a schematic of the network composed of elements *a*, *b*, *c*, and *d*. On the right is a diagram of *a* as it appears in the machine's memory. As the punched cards describing *a* are read in, the input program stores synapse parameters and transit time in a 10-cell block uniquely assigned to *a*, and forms an associative list in which the location and pulse energy of each terminal is recorded. The order in which terminals are listed is irrelevant. The *last* entry of the list always has a special symbol in its associative field to indicate termination of the list.

Thus *network topology* is represented by a set of variable length associative lists, one per element, with no rigid restrictions on the number of connections a given element may have.

The heart of the Network Simulator is a table of future events which *controls the sequence in which active elements are scanned.* We call this the "Next Instant Table," or more briefly "NIT," and it is an associative list. Its role can best be explained with reference to the basic simulation loop as shown in the flow chart of Figure 1. If we let the simulation interval be ΔT, then every ΔT seconds in the organism's life network simulation ceases and the interaction of organism and environment is computed. One pass through the whole program (Network Simulator + Interaction Simulator) is made every ΔT of the organism's life. If we think of a clock that indicates the passage of time for the organism, then we can say that this clock advances only during the simulation process in Box B.

In Box A the frequency of firing of each Receptor, as a function of stimulus, is computed. Since the stimulus is assumed to be constant over the coming ΔT, the frequency is likewise constant. For each Receptor, an entry is inserted in the NIT for each output pulse over the next ΔT. For example, if Receptor "r_1" is firing at 60 pulses per second, and ΔT is one second, then 60 entries will be made in the NIT, one for

USE OF 2-LAYER ASSOCIATIVE LIST FOR REPRESENTATION OF NETWORK TOPOLOGY

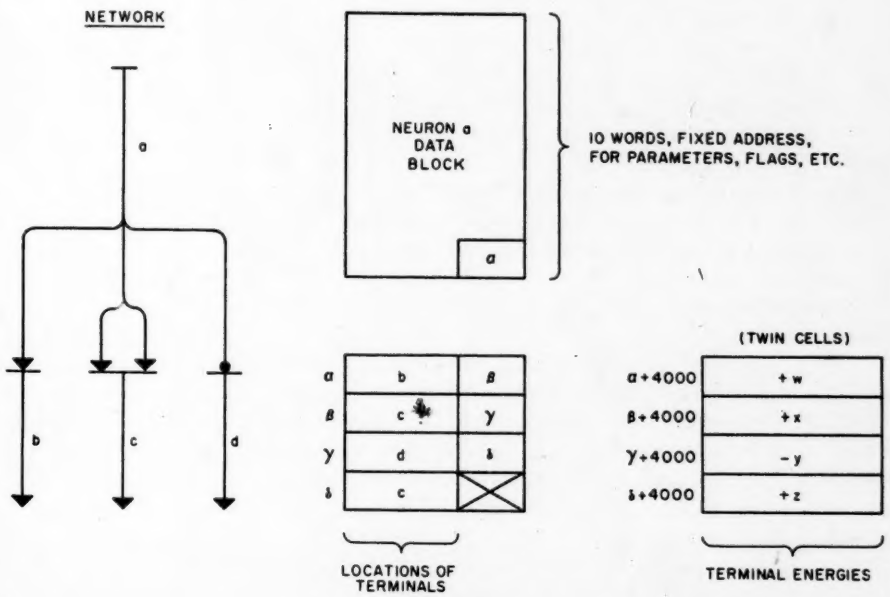

FIGURE 7

each output pulse, ordered by predicted time of occurrence, with each entry containing the name "r_1" and the particular time involved. The *first* output pulse of a Receptor is not assumed to occur at the very beginning of the simulation interval, but rather is delayed by the product of a pseudo-random number and the period of that Receptor. Thus the *phase* of the whole train of Receptor pulses, relative to the beginning of the simulation interval, is randomly chosen. A new random number, always ranging between 0 and 1, is computed for each Receptor.

In this manner all Receptor outputs for the next ΔT are entered in the NIT. They will of course be intermixed, according to time, with each other and with whatever output pulses (of Inter-neurons) were left over from the previous simulation interval, i.e., if the synapse of an Inter-neuron fires near the end of a simulation interval, its transit time may be sufficient to delay the appearance of its output pulses until the next simulation interval. At the end of a simulation interval, the NIT is preserved, thus preserving network activity from one interval to the next.

After all receptors have been accounted for, control moves on to Box B (of Figure 1). The organism's time now begins to move forward. The first entry in the NIT is picked off (deleted from the list) and the "clock" advanced to the time specified in that entry. All the output terminals of the element named in this entry are fired, one by one, on their respective synapses. If one of these synapses is thereby caused to fire, then a new entry is immediately made in the NIT specifying the name of the newly active element and the time at which *its* terminals will emit pulses. The first entry in the NIT has now been removed and processed. The next entry is picked off, the clock advanced to its time coordinate, and the above process repeated. Thus the program of Box B works its way through the NIT (sometimes creating several new entries for every one it removes), consuming entries in the order in which they appear until the clock has been advanced to the end of the simulation interval. If most elements are firing at low frequencies there will not be many entries in the NIT spanning ΔT. The NIT will therefore be consumed rapidly and the *simulation rate* will be *high*. If a number of elements are

A GRAPHICAL REPRESENTATION OF THE NEXT INSTANT TABLE (NIT) DURING SIMULATION OF A SIMPLE NETWORK

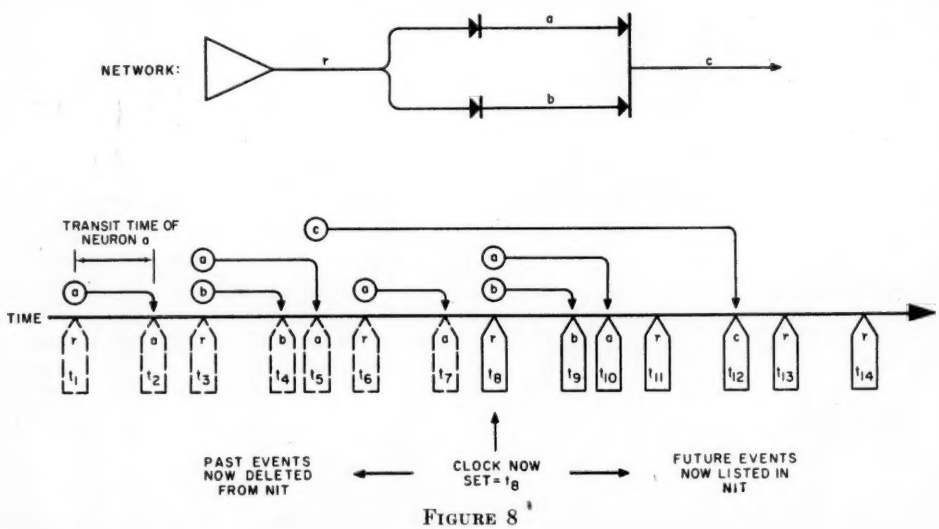

FIGURE 8

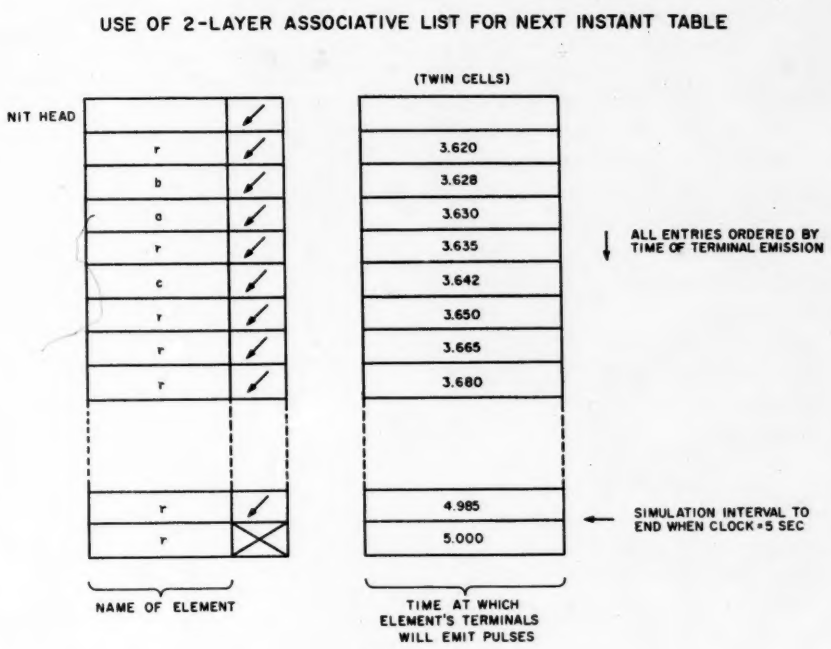

FIGURE 9

firing at high frequencies, the NIT will be large, and the simulation rate *low*.

A tally of the number of times an element has fired during the simulation interval is maintained in the 10-cell block assigned to that element. At the end of a simulation interval, control moves on to Box C of Figure 1 where the tallies of the Efferents are converted to mean frequencies. These frequencies then determine changes in length of Muscle contractile elements via interpolation tables.

Figure 8 is an attempt to pictorially show the use of the Next Instant Table. The horizontal line is a fragment of time in the life of the network shown above. The large vertical markers represent entries in the NIT, each with its time coordinate, t, and the name of the element whose terminals will fire at t. The synapse parameters of elements a, b, and c are assumed here to be such that the firing of r always results in the firing of a, and through temporal summation, every two pulses from r cause b to fire. Furthermore, if a and b fire sufficiently close together, the temporal summation will fire c. Immediately above the time line, a circle around an element's name indicates that its synapse fired at that instant. The horizontal arrow defines the transit time, and points to the time at which that element's terminals fire, necessitating a corresponding entry in the NIT. The Receptor entries, occurring at regular intervals equal to the period between firing, were all inserted at the beginning of the simulation interval. The firing of a, b, and c are determined, however, as simulation proceeds from entry to entry. The clock is shown set at t_8. The program has just discovered that r's terminals caused both a and b to fire, and appropriate entries have been made at t_9 and t_{10}. The entry at t_8 will now be discarded (i.e., will be returned to the Availability List) and the clock will be advanced to t_9 when the next entry is processed.

Figure 9 shows the NIT as it would appear in the machine's memory just before the receptor entry at t_8 is discarded. The second layer of the associative list holds the actual values of t_8, t_9, t_{10}, etc. Note that the time interval between successive Receptor entries is 5 milliseconds. This means that at the last sampling of the environment, the stimulus

at r was sufficient to cause r to fire at 1/.005, or 200 pulses per second.

It might be observed, in conclusion, that the problem of simulating our asynchronous nerve-net model is related to the "queuing" problems dealt with in the field of Operations Research. The synapse model we use might be likened to a non-scheduled airlines terminal. The incoming pulses are short-tempered customers who arrive at odd times and who leak away (leave) in a certain period of time if there are not enough other customers to justify a flight out (synapse firing). Such analogies, if not instructive, are at least amusing.

REFERENCES

Bullock, T. H. Neuron doctrine and electrophysiology. *Science*, 1959, 129, No. 3353.

Culbertson, J. T. *Consciousness and behavior*. Brown, 1950.

Culbertson, J. T. Some uneconomical robots. In Shannon & McCarthy (Eds.), *Automata studies*. Princeton, N. J.: Princeton, Univ. Press, 1956.

Crichton, J. & Holland, J. A new method of simulating the central nervous system using an automatic digital computer. Willow Run Laboratory Report 2144-1195-M, Univ. of Michigan, March 1959.

Farley, B. G. & Clark, W. A. Simulation of self-organizing systems by digital computer. *Trans. of I.R.E.*, PGIT-4, Sept. 1954.

Farley, B. G. & Clark, W. A. Generalizations of pattern recognition in a self-organizing system. Proceedings 1955 Western Joint Computer Conference.

Frankel, S. On the design of automata and the interpretation of cerebral behavior. *Psychometrika*, 1955, 20, No. 2.

I. R. E. Symposium. The design of machines to simulate the behavior of the human brain. I.R.E. Trans. on Electronic Computers, Dec. 1956.

Kleene, S. C. Representations of events in nerve nets and finite automata. In Shannon & McCarthy (Eds.), *Automata studies*. Princeton, N. J.: Princeton Univ. Press, 1956.

McCulloch, W. S. & Pitts, W. A logical calculus of the ideas immanent in nervous activity. *Bull. Math. Biophysics*, 1943, 5, No. 4.

McCulloch, W. S. The brain as a computing machine. *Electrical Engineering*, 68, No. 6. 1949. (a)

McCulloch, W. S. Digital computers called brains. *Scientific Monthly*, Dec. 1949, Vol. 69. (b)

McCulloch, W. S. Why the mind is in the head. In L. Jeffress (Ed.), *Cerebral mechanisms in behavior*. New York: Wiley, 1951.

McCulloch, W. S. Biological computers. I.R.E. Trans. on Electronic Computers, EC-160, Sept. 1957.

Newell, A. & Shaw, J. C. Programming the logic theory machine. RAND Publication P-954, 1957.

Newell, A., Shaw, J. C., & Simon, H. A. The processes of creative thinking. RAND Publication P-1320, 1958.

Newell, A., Shaw, J. C., & Simon, H. A. A report on a general problem solving program. RAND Publication P-1584, 1959.

Rashevsky, N. *Mathematical biophysics*. Chicago: Univ. of Chicago, 1948.

Rochester, N., Holland, J., Haibt, L. & Duda, W. Tests on a cell assembly theory of the action of the brain, using a large digital computer. *Trans. of I.R.E.*, Vol. IT-2, No. 3, Sept. 1956.

Shaw, J. C., Newell, A., Simon, H. A., & Ellis, T. O. A command structure for complex information processing. Proceedings 1958 Western Joint Computer Conference.

Solomonoff, R. J. An inductive inference machine. 1957 I.R.E. Convention Record, Part 2, March 1957.

Von Neumann, J. The general and logical theory of automata. In L. Jeffress (Ed.), *Cerebral mechanisms in behavior*. New York: Wiley 1951.

Von Neumann, J. Probabilistic logics and the synthesis of reliable organisms from unreliable components. In Shannon & McCarthy (Eds.), *Automata studies*. Princeton, N. J.: Princeton Univ. Press. 1956.

Von Neumann, J. *The computer and the brain*. New Haven, Conn.: Yale, 1958.

Weaver, W. Science and complexity. *American Scientist*, 1948, 36, No. 4.

Wiener, N. *Cybernetics*. New York: Wiley, 1948.

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ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members [redacted] Staff of the Mental Health Research Institute

BLATT, SIDNEY J., AND STEIN, MORRIS I. **Efficiency in problem solving.** *J. Psychol.*, 1959, 48, 193–213.

The authors define certain variables of performance in the PSI apparatus. (For description of the apparatus and its use as a problem-solving device, see John, Erwin Roy, & Miller, James G., The acquisition and application of information in the problem-solving process: An electronically operated logical test. *Behav. Sci.*, 1957, 4, 291–300; and Gyr, John W., An investigation into, and speculation about, the formal nature of a problem-solving process. *Behav. Sci.*, 1960, 5, 39–59.)

In particular, efficiency is defined in terms of the "absence of unnecessary questions" of which several categories are mentioned. Another variable is the *A-S* shift value, which measures the clarity of the division between the phase in which predominantly analytic questions and the phase in which predominantly synthetic questions are asked. (The *A-S* shift value is not to be confused with the *position* of the *A-S* shift, which appears in other studies. Also the authors' definitions of analytic and synthetic questions are related to but not identical with those offered by other authors.) Other variables relate to phases of performance, some identical with, others slight modifications of analogous phases described elsewhere. The findings consist of (a) correlations of the efficiency measure with other performance variables and (b) correlations between various indices of personality and performance variables. Among the latter, the large and significant ($< .01$) correlations appear to be: (1) *A-V-L* aesthetic scale vs. efficiency (+.50); (2) *A-V-L* aesthetic scale vs. relative length of initial (fact gathering) phase (time: +.56; questions: +.50); (3) *A-V-L* aesthetic scale vs. *A-S* shift value (+.49); (4) *A-V-L* aesthetic scale vs. rate of questions (−.49); (5) Taylor anxiety scale vs. relative time of initial phase (−.41); (6) Freeman anxiety scale vs. repeated questions (+.32, $p = .05$).

The authors discuss the possible psychological significance of these correlations. (*AR*)

HARARY, FRANK. **Status and contrastatus.** *Sociometry*, 1959, 22, 23–43.

Viewing the structure of an organization as a directed graph, the author defines *status* and *contrastatus* of a member in terms of his hierarchical position on the graph. Thus the status of individual A is defined as

$$S(A) = \sum_{k=1}^{m} k a_k ,$$

where a_k is the number of subordinates of A, k levels removed. If the directions of all the arrows of the digraph are reversed, the status of an individual in the resulting graph is his contrastatus in the original graph. Roughly, then, the status of a member of an organization is measured by the amount of authority he wields (in the formal structure), while his contrastatus is measured by the amount of authority bearing down upon him.

Total status, defined as the sum of the individual statuses, and total contrastatus emerge as group parameters.

Theorems pertaining to calculation of these and related parameters are proved, among them the following: Among all organizations with n members, there is a unique structure for which the total status is maximal. The digraph of this structure consists of n points and n lines arranged in a cycle.

Organizations can also be constructed in which all members have equal status, as defined above (peer groups). Having defined

an automorphic group (one in which every point can be transformed into every other point by an automorphism), the author proves that not every peer group is an automorphic one.

The mathematical definitions and the deduced properties of digraphs being perfectly general with respect to structure consisting of elements and non-symetric relations, other applications suggest themselves.

In particular, the author mentions the "publication status" as some sort of index of creativity. "The first-order references can be considered as the immediate subordinates of a paper; the second-order reference as the 2-subordinates, etc." (*AR*)

GREGORY, IAN. **Genetic factors in schizophrenia.** *Am. J. Psychiat.*, 1960, 11, 961–971.

Two main approaches have been used to examine the genetic causation of "functional" psychoses: the investigation of twins including at least one affected member, and the estimation of frequencies of abnormality in various classes of relatives. More extensive data of both sorts have been gathered on schizophrenia than any other form of functional psychoses probably due to its relatively high frequency of occurrence, and the high degree of ascertainment of schizophrenia.

The twin studies make use of the zygosity of twins in attempting to establish the hereditary hypothesis. The author feels that the previously reported higher incidence of schizophrenia in identical as opposed to fraternal twins may be explained by faulty diagnosis of schizophrenia, by faulty diagnosis of zygosity due both to the unequal probabilities of ascertaining the two types of twins and the relatively primitive confirmation methods at the time of many of the studies, by counting twins twice if the abnormal individuals were detected independently, by biological bias with prenatal or postnatal onset, and by greater environmental similarities for identical than for fraternal twins.

Familial studies have resulted in four hypotheses concerning inheritance of schizophrenia: simple recessivity with incomplete penetrance and expressivity, simple recessivity with incomplete manifestation in homozygote, partial dominance with complete manifestation in homozygote but incomplete manifestation in heterozygote, and partial dominance with homozygous manifestation. The author rejects all of these hypotheses either on grounds of poor corroborative evidence or on the grounds that necessary assumptions have not been satisfied. The most striking of the latter are the assumptions of randomness and gene stability, in a Hardy-Weinberg equilibrium, which underlie all of the previous hypotheses and which are not met in the case of schizophrenia.

Since no simple Mendelian hypothesis is compatible with all the data extant on the occurrence of schizophrenia, the author investigates the following non-mutually exclusive hypotheses: (1) That schizophrenia is predominantly determined by environmental influences. (2) That the genetic component of the schizophrenic syndrome is heterogeneous. (3) That at least part of the genetic component of schizophrenia is polygenic in nature.

Statistical association between schizophrenia and limited intelligence, inferior socio-economic status, maternal mortality, and immunological or other somatic characteristics of schizophrenics, are all compatible with these hypotheses. (*GR*)

WEISS, PAUL. **Knowledge: A growth process.** *Science*, 1960, 131, 3415, 1716–1719.

In the higher animal, growth converts food from the environment into body substances in a sequence of four major steps: intake, digestion, assimilation, and utilization. In the last step, cells branch in two directions, reproduction or manufacture of special products. This course is closely mirrored in the growth of scientific knowledge. Experience (environment) furnishes an input of raw data (food stuffs) which is digested with the separation of ordered data (nutrients) from discards (waste products), assimilated into hypotheses (intermediary products), and utilized into knowledge (protoplasm) which can in turn lead either

to useful results (functional products) or more knowledge (more protoplasm).

This analogy leads to two major arguments. In the first place it shows that information is not tantamount to knowledge. Information gathering is no more knowledge than the stuffing of a hamster's pouch is growth. Both the educator and the editor should take this point to heart. It is not enough to teach the young scientist the present state of knowledge, but also how knowledge has gotten to its present point, as this is the only way in which scientific knowledge can expand further. The editor should encourage rather than blue-pencil interpretive excursions, thus fostering the growth of knowledge rather than the accumulation of bulk.

This first argument has the danger of emphasizing generalizability at the expense of those items which remain useful through their individuality, just as hormones, despite their common name, have specific functions. Taking the references cited in journals of different biological disciplines as a guide, it becomes evident that the usefulness of general as opposed to particular information is a function of the particular discipline being observed. However, there is a rapid attrition of all unassimilated information. It is suggested that, in order to alleviate the current glut of informational data, manuscripts be evaluated as to their prospective life span, and published in either fast-aging or slow-aging serials.

Graded in terms of relevance, not every observation is worth reporting; not every report is worth recording; not every record is worth publishing; and not every publication is worth preserving for eternity, except in sample specimens as in Noah's Ark. The process of the growth of scientific knowledge is one which often thrives more on a spare than an overly rich diet, and in which self-restraint can readily ward off obesity. (*GR*)

STORY, J. L., MELBY, JR., J. C., EGDAHL, R. H., AND FRENCH, L. A. **Adrenal cortical function following stepwise removal of the brain in the dog.** *Am. J. Physiol.*, 1959, 196, 583–588.

While a strong body of evidence exists indicating that the hypothalamus markedly influences adrenalcortical function, the identification of the critical loci involved is a matter of dispute. The authors have employed the Hume-Nelson technique for measuring 17-hydroxycorticoid production in conjunction with stepwise ablation of the CNS to determine whether higher structures are required for ACTH production and whether ACTH response can occur without lower centers as well. The brain was removed by suction in the order (1) cerebral cortex, striatum, hippocampus, fornix, and septal region to the level of the anterior commissure; (2) thalamus to the massa medians and hypothalamic sulcus; and (3) hypothalamus between the superior colliculus and the infundibalar region.

The authors' results indicate that neocortical and subcortical structures above the hypothalamus are not required for adrenal secretion of 17-hydroxycorticoids. Corticoid secretion also continued for up to six hours *after removal of the body of the hypothalamus.* Whether this interesting finding is due to the activity of the few hypothalamic nuclei probably remaining in this preparation, a corticotropin-releasing humor stored in the pituitary stalk or the pituitary proper, the gradual release of some bound form of corticotropin within the adrenal, or some other mechanism has not yet been determined. (*AY*)

DECSI, L. AND MÉHES, J. **Increased sensibility of the hypothalamic cells to the uncoupling effect of tranquilizing drugs.** *Arch. Internat. Pharmacodyn. Thér.*, 1959, 119, 294–304.

Knowledge of the mechanisms whereby psychopharmacological agents exert their effects clearly would be of enormous value in establishing the biochemical and pharmacological correlates of behavior. In the present paper, the authors have extended their earlier work on the uncoupling of oxidative phosphorylation by chlorpromazine to similar *in vitro* studies of meprobamate and benzactyzine. As in the case of

chlorpromazine, the authors present evidence that whole homogenates of nervous tissue are more sensitive to the uncoupling action of these agents than equivalent homogenates of liver tissue and that hypothalamic tissue is particularly sensitive. Captodiamin showed a similar selective effect on nervous tissue as a whole but inhibited oxidative phosphorylation of all CNS regions about equally. None of these compounds had significant inhibitory action on oxygen uptake. Under the conditions employed and using pyruvate as substrate, the highest P/O ratio that could be obtained by these authors was 2.20.

The authors also mention that meprobamate and benzactyzine, like chlorpromazine, inhibit ATPase activity of brain cells while captodiamin does not. The authors conclude that chlorpromazine, benzactyzine, and meprobamate probably exert tranquilizing effects by the same mechanism, presumably via their influence upon oxidative metabolism. Other well known effects of these compounds upon biochemical systems are not considered in this paper. (*AY*)

Much madness is divinest sense
To a discerning eye;
Much sense the starkest madness.
'Tis the majority
In this, as all, prevails.
Assent, and you are sane;
Demur,—you're straightaway dangerous,
And handled with a chain.

EMILY DICKINSON, "Much madness is divinest sense"
The Oxford Book of American Verse

ABOUT THE AUTHORS

Bernice T. Eiduson ("Values of Psychiatric Patients"), chief psychologist at the Reiss-Davis Clinic for Child Guidance (Los Angeles) and author of six articles published within the past three years, is also author of the book *The Myth-Breakers*, now in press. This book is concerned with the psychology of research scientists and is representative of Dr. Eiduson's present interest in research in the field of creativity, particularly the psychology of persons who go into creative fields. Regarding other research presently in progress, she says, "Because I have been in clinical psychology for the last fifteen years, and know only too well on what a loose theoretical structure our diagnostic and therapeutic tools rest, I have become interested in seeing what the new developments in the biological, neurophysiological, and biochemical sciences can contribute to our understanding of clinical behavior, and conversely what our personality data can suggest in the way of new research attacks for these sciences. My interdisciplinary mediator for my excursions into the natural sciences is my husband, who is a biochemist doing research on the brain. He, two other biochemists, and I are currently working on a book, *Biochemistry and Behavior*, which Van Nostrand is publishing." Dr. Eiduson received her B.A. and M.A. degrees from the University of Buffalo and a Ph.D. from the University of California at Los Angeles in 1957.

Joshua A. Fishman ("A Systematization of the Whorfian Hypothesis"), a graduate of the University of Pennsylvania and a Columbia Ph.D., is Dean and professor of psychology at the Yeshiva University Graduate School of Education. He has published over fifty articles dealing with American minority groups, language and communication, and American higher education. Dr. Fishman is presently conducting research in non-English language resources of American ethnic groups (sponsored by the U. S. Office of Education, Language Development Section), social and psychological theory for selection and guidance of college applicants, and suburban interracial housing and community patterns. He defines his great interest in minority groups, language and communication, and education relative to the Whorfian hypothesis in the following statement: "As a social psychologist primarily interested in what are commonly considered to be three unrelated topics (American minority groups, language and communication, and education), I was long unable to understand why the Whorfian hypothesis had such an attraction to me. Lately I have begun to realize that it offered a rare opportunity to combine all three of my interests. Much of the language loyalty of American ethnic as well as other groups is prompted by the conviction that their languages are unique. Whorf attempted to define the uniqueness of individual languages. Whatever uniqueness may exist in terms of cognitive impact may or may not be counteracted by educational efforts."

Llewellyn Gross ("System-Construction in Sociology"), professor and chairman of the department of sociology at the University of Buffalo, received his formal education at the University of Minnesota where he obtained his B.A., M.A., and Ph.D. degrees. He has conducted research in and has written articles on measurement, typology, classification, etc.; his most recent papers being "Theory Construction in Sociology: a Methodological Inquiry" and "An Epistemological View of Sociological Theory." In discussing his intellectual development, Professor Gross writes that it has "fluctuated between interest in the systematic and analytic on the one side and the interpretative or imaginative on the other." He has strong misgivings for critics who insist that everything of value is found in only one approach. He adds, "To me, our greatest mistakes have been to assume that any piece of research which introduces mathematics or logic must use such methods and only such methods at their most advanced levels, and that anyone who prefers to write in the metaphors of literature or common sense must, in no way, attempt to simulate the exact procedures of natural science." His present paper is the third in a series of recent publications in which he attempts to "bridge the concerns that characterize these contrasting viewpoints." He further states, "Without research on the middle regions that join the numerous antinomies of our collective endeavors, we cannot maintain the integrity of our chosen field(s). Consequently, I am preparing to suggest this image as a postulational (epistemological and methodological) principle for guiding the whole of social research. In this connection, I am attempting to revamp the historical methods of dialectic with the aim

of providing a set of perspectives for 'deciding' various issues in behavioral science."

Lawrence A. Kratz ("The Motivation of the Business Manager") is an assistant professor of economics at Mississippi State University, with past experience at the University of South Carolina and at Franklin and Marshall College where he taught economics and geography. He has also had business experience as a municipal bond analyst with Dun and Bradstreet in New York City. Professor Kratz writes about himself, "I am an Ohioan who has been teaching economics (Cincinnati School) at Mississippi State University since the year of the Supreme Court Desegregation Decision. This article on 'The Motivation of the Business Manager' grows out of a four-year argument between me and the rest of my department about orthodox marginal productivity analysis and the profit maximizing assumption. It is my modest hope that this article will set the feet of economic theory on the path of non-marginal righteousness." Author of several articles on geography, Professor Kratz is presently conducting research in economic motivation, and has a great interest in Christian theology. A graduate of the University of Cincinnati with B.A. and M.A. degrees, he also attended Ohio State and Mississippi State Universities for further graduate work.

Robert A. LeVine ("The Role of the Family in Authority Systems: A Cross-Cultural Application of Stimulus-Generalization Theory"), assistant professor of anthropology, Committee on Human Development, the University of Chicago, has a doctorate in social anthropology from Harvard University, and an M.A. and B.A. from the University of Chicago. Regarding present research, Dr. LeVine writes, "I am engaged in selecting a sample of societies on which to test the hypotheses presented here and constructing rating scales for this purpose and for the testing of other hypotheses relating socialization to cultural behavior." His interest in political socialization " . . .stems from work on the psychological basis of social control done first at Harvard and later among the Gusii of Kenya, in conjunction with the comparative study of socialization directed by John W. M. Whiting of Harvard, Irvin L. Child of Yale, and William W. Lambert of Cornell. It also stems from my experience as an anthropologist in the political science department at Northwestern [where he taught previously]. My theoretical approach to the subject combining psychoanalytic concepts, behavioristic learning theory, and the comparative study of social structure was greatly influenced by Whiting, as was my preference for the quantitative cross-cultural method for testing hypotheses." Among articles Dr. LeVine has published relative to the above research are "An Attempt to Change the Gusii Initiation Cycle" and "Anti-European Violence in Africa: A Comparative Analysis." His most recent work "The Internalization of Political Values in Stateless Societies" is in press.

Bernhardt Lieberman ("Human Behavior in a Strictly Determined 3 × 3 Matrix Game") is a research fellow at the Center for International Affairs at Harvard University, where with Professor Thomas C. Schelling he is studying interaction between two or more individuals in situations of conflict. "The situations will be games which will permit threat, bargaining, and cooperation while allowing only limited communication." From September, 1958 to August, 1960 he was a post-doctoral research fellow of the National Institute of Mental Health in the department of social relations at Harvard where he studied behavior in situations described by the von Neumann & Morgenstern game theory models. Initial work in this study was a basis for his present article. He recently wrote and published (with co-author Philip Nogee) "The Auction Value of Certain Risky Situations." Dr. Lieberman obtained his doctorate in social psychology from Boston University in 1958, his M.A. from New York University in 1952, and his B.A. from Brooklyn College in 1950.

Charles Morris ("Values of Psychiatric Patients"), graduate research professor in philosophy at the University of Florida, received a B.S. degree in psychology at Northwestern University in 1922 and a Ph.D. degree in philosophy from the University of Chicago in 1925. Editor of *Mind, Self, and Society* and of *Philosophy of the Act* (both by George H. Mead), he is associate editor of the *International Encyclopedia of Unified Science*. A fellow of the American Academy of Arts and Sciences and past fellow of the Center for Advanced Study in the Behavioral Sciences, Dr. Morris was invited to represent philosophy on the Joint Commission for Mental Illness and Health. In the study required as a member of the commission, he noted various statements suggesting a relationship between mental health and value conflicts. Having worked in the area of values for many years using quantitative techniques, he extended this study into the values of psychiatric patients. It was while attending a conference at the Hacker Foundation that he came in contact with Dr. Eiduson and they decided to cooperate on this project. Dr. Morris is the author of several books and is currently interested in "The relations of

signs to values, or as I prefer to put it, the relation of signification to significance."

Denis O'Donovan ("Values of Psychiatric Patients") received his Ph.D. degree from the University of Florida this past June (1960), an M.S. degree from the University of Miami, and his B.A. from Duke University. He is currently assistant professor of psychology at the University of Missouri. Although this is his first published article, he has presented two papers which grew out of this study, one at the 1959 Southeastern Psychological Association convention and the other at the 1959 American Psychological Association meetings. "Subliminal Operant Conditioning" was presented at the 1960 Florida Psychological Association meetings and "The Value of Valuing" will be presented at the 1960 APA convention. Regarding current interest and research, Dr. O'Donovan comments, "I have been attempting to comprehend in my own mind a system in which both Maslow and Skinner play a role. In this, I have been aided by Razran's latest theorizing. My first attempt is a so-far decidedly unpublished paper, 'Beyond the Reinforcement Principle,' which asserts that learning takes place under the dominance of different stages of learning laws predictable on the basis of genetic and situational variables. I'm interested in investigating why reinforcers reinforce." Other research interests include the problems and possibilities of gifted students; the general problem of values; an approach to individual, situational, and group differences in structure of judgments; and subliminal operant conditioning.

Richard F. Reiss ("The Digital Simulation of Neuro-Muscular Organisms") was elected to Phi Beta Kappa and graduated Magna Cum Laude with an A.B. in psychology from the University of Southern California in 1952 (where he also studied for two years in the Graduate School of Philosophy). He is presently employed as research associate in the Laboratory for Automata Research at Librascope Division of General Precision, Inc. Events and experiences responsible for Mr. Reiss' present interest and work in the computer field are best described by Mr. Reiss himself: "After several years in the Navy as an electronics technician, I entered college as a physics major. By my senior year, I had shifted to a psychology major with a physics minor, partly as a result of courses given by J. T. Culbertson on nerve-net theory and cybernetics. After graduation, I was engaged in electroencephalography research, under Dr. John Kennedy, for a year at the RAND Corporation. During my second year of graduate work in philosophy (mainly logic and philosophy of science), I began working as a programmer at the Electrodata Company (now Burroughs). My interest in computers soon overwhelmed my academic work, and since 1955, I have been working in the computer field—returning to RAND for work on SAGE air defense at Lincoln Lab., then to Atomic Energy Division of the Babcock & Wilcox Company where automatic programming was my main concern, and finally to Librascope to help form the Laboratory for Automata Research. Our central goal here is to acquire knowledge that may eventually lead to the creation of a race of 'intelligent machines' of many varieties (in the period, perhaps, of 1970–1990). I am, of course, interested in any and every field that may have something to contribute; it is a frightening, but stimulating, fact that almost every area of science and technology is in one way or another relevant to our goals. Biology presents a bewildering array of relevant data and theory, and this simulation model represents but one of several projects we have which cuts across biology, psychology, and automata theory."

We feel in one world; we think and name in another. Between the two we can set up a system of references but not fill the gap.
The Maxims of Marcel Proust

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